MEMOIRS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

MANCHESTER.

MEMOIRS

OF THE

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AND

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OF

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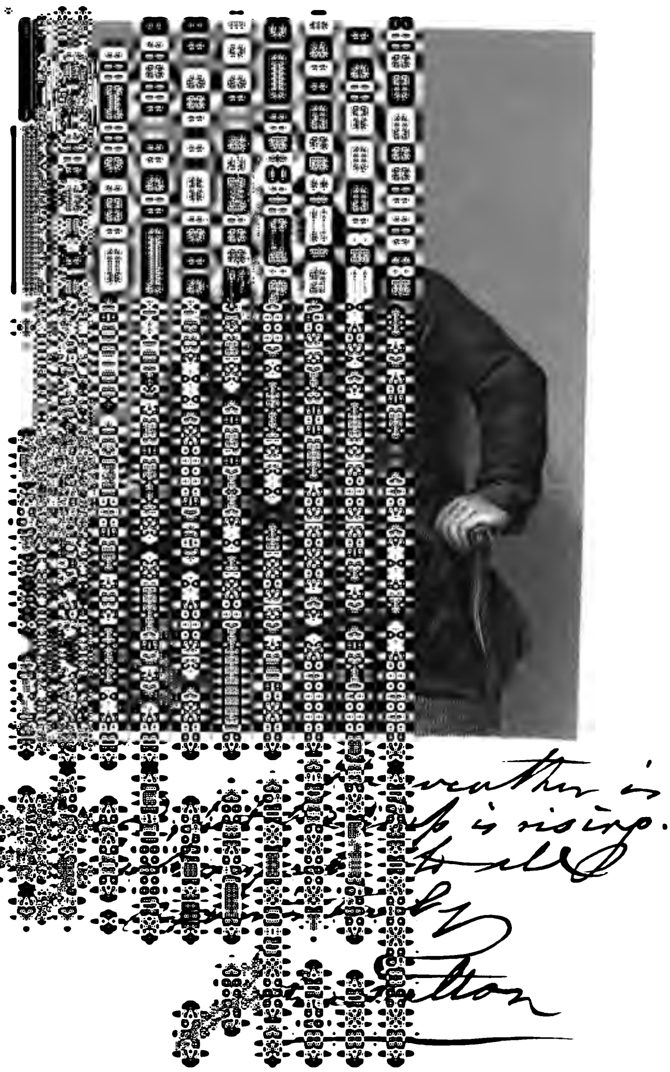

MEMOIR

OF

JOHN DALTON,

D.C.L., F.R.S.,
INSTIT. (ACAD. SC.) PARIS. SOCIUS.,
PRESIDENT OF THE LITERARY AND PHILOSOPHICAL SOCIETY OF
MANCHESTER, ETC., ETC.;

AND

HISTORY

OF

THE ATOMIC THEORY

UP TO HIS TIME.

BY

ROBT. ANGUS SMITH, PH.D., F.C.S.

SEC. TO THE LIT. AND PHIL. SOC.

LONDON:
H. BAILLIERE, 219, REGENT STREET,
AND 290, BROADWAY, NEW YORK.
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NOTE.

In this, as in the previous Volumes, the Society is not to be considered responsible for any statements or reasonings offered.

PREFACE.

THE life of Dalton has already been written, but chemical literature seemed to demand a more minute history of the atomic theory up to his time, without at all disparaging the valuable history of chemistry by Dr. Kopp, or the work of Dr. Daubeny, which treats principally of the more modern part. For this and reasons elsewhere mentioned, I have made the distinctive feature of the volume the history of our ideas of matter bearing on modern chemistry, until the time when Dalton flourished. There is a short memoir which breaks off at the fourth chapter, or the time when Dalton first published on atoms, in order to begin the general history, which again leads to Dalton at the eleventh chapter.

Mr. James Woolley was kind enough to lend me all the papers relating to Dalton which he possessed, together with his own MS. memoir, and Mr. Giles, with similar kindness, lent me the memoir which he had written. Dr. Henry's volume also has not been

neglected. Mr. Isaac Dickenson, of Cockermouth, and Mr. Thomas Bewley, of Bassenthwaite, have also furnished me with interesting letters.

It might have been expected that more attention would have been given to Dalton's private life, living, as I am, among so many who knew him, but none with whom I have conversed have given any important information not here embodied. I considered also that Dr. Henry had very fully treated that subject, and that it would be unwise and wanting in respect to go over the same ground exactly, even when the same materials were supplied, I have therefore been minute only in such things as did not appear to me elsewhere treated, or such as seemed the most characteristic according to my ideas.

The history of Dalton's many scientific inquiries on subjects other than the atomic theory, is given so shortly that it might almost have been left out, did it not give a greater completeness to the memoir, for the use of such as read no other life of the same man. The history of our ideas of matter is one of the most interesting "fairy tales of science," it is a pity that, like so much else in science and philosophy, it should be so frequently spoiled by dryness. The desire to avoid this has led me to extend further than usual the meaning of the title. The plan of quotation instead of description has been adopted, as the most just as well as the most interesting, if well managed, although one which may gradually be allowed

to degenerate into mere *extractive* matter. Every worker and thinker is allowed to speak for himself, and there are few allusions made to the opinions of any, however eminent, who has not himself laboured on the subject. In the court of science every man is his own best witness; by using description instead of quotation we employ advocates who bandy about the meaning, until at last it can nowhere be found. We cannot be too careful of the fame of the absent, were it merely to protest against the loud assertion of self, which is so easy for those who are present, even when their only hope of fame lies in that two-edged truth, that "a living dog is better than a dead lion."

ERRATA.

Near bottom of page 77, the inverted commas should begin, "*For one is not the same.*"

Page 79, for *air and matter*, read *air and water*.

Page 251, for *John Gough W.*, read *Henry Hough W.*

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MEMOIRS

OF

The Literary and Philosophical Society of Manchester.

MEMOIR OF DR. DALTON,

AND

HISTORY OF THE ATOMIC THEORY.

[*Read October 2nd,* 1855.]

CHAPTER I.

In 1789, when this Society was first established, chemistry could not with propriety be called a science, although Lavoisier was attempting to decide on some of its more prominent laws, and although Cavendish, Black, and Watt had raised it from that position of obscurity to which the meagreness of its results had so long condemned it, and shewn to the world that it possessed a power, apparently the highest in order. With the exception of these and a very few others, the whole body of its students were under the subjection of one of the strangest delusions that has ever usurped the place of a law of nature. A body of men for many ages at work had made so little progress towards eliciting definite forms of thought upon the elements with which they worked, that the theory of Phlogiston was regarded as a great discovery; a fanciful theory founded on an explanation

of facts confessedly incomplete in the eyes of the most enlightened, was made the great centre round which all chemical knowledge placidly rolled. We may be allowed to boast, that, when this society had existed only a few years, one of its members was found to devise a theory which not only was sufficient to throw light on the past, but at once to put into the infinite observations of his predecessors that reason and order of which before they seemed totally deprived. The hermetic mystic and alchemist had toiled over the difficulties, had tried to remove them by physics and by metaphysics, had explained them by supposing an independent will in the elements or by the immediate Divine interposition; by their wildest imaginings and their clearest reasonings little definite had been attained. The laws which Dalton shewed us to be dominant in matter, considered chemically, were at once clear enough to satisfy the most exact reasoner, great enough to satisfy the most poetical thinker, and simple enough to satisfy those who believe that, at least the great primary laws of nature are simple, whether because the highest wisdom can of course attain its ends with the greatest ease, or because the simplest germ is more easily fitted to branch out into an endless development of character and power.

It is scarcely possible to write the life of Dalton without referring to this Society, and as it is by request of this body that I have undertaken to write, I must explain to them what I have specially attempted to do. After Dr. Henry had written the life of Dalton, it might fairly be asked why I should undertake one. I was requested to write this memoir at a time when it was uncertain when Dr. Henry's would appear. I was unwilling to compete with Dr. Henry, and saw no propriety in doing so, although he offered me his materials on certain fair conditions. I had no intention to write a complete life, nor, I believe, had the society the desire to have a memoir so large as that which I present, and I

considered it better to confine myself to certain aspects of Dalton's life and discoveries, thus preventing any reason for asking why another memoir should have at all appeared. The portion which I have most carefully worked out is the History of the Atomic Theory. Much has been said of it; some have given the credit to Dalton, some have taken it from him; most writers have confusedly mixed him up with others. Some have looked forward to the probable developments of truth in after times, and undervalued the laws of combination as they now stand, and with them the discoverers. It has been my desire to shew distinctly what of importance each celebrated thinker or worker has said or done in the matter before Dalton, and what he has himself accomplished. This is done by bringing the original words of the authors, and endeavouring to find what amount of meaning can be attached to them. As for all future developments of the present laws I can only say that, believing as I do in the infinite wisdom with which creation is ordered, I am ready to believe in infinite developments of any law; but strange as may be the new and possible combinations of elements, and interesting also the breaking up of these our present elements into other elements, or even into mere pieces, it is not to be imagined that even nature has any thing in store for us fitted to answer the same purpose within the same limits and still simpler and more extensive than the laws of combination as Dalton has expressed them.

Of the man it has been attempted to give only a short sketch, and the whole merely to serve as an enlarged epitaph, written here instead of on his tomb, a token of remembrance for ourselves especially, like the coffin of some departed friend to preside at our feasts, and as a contribution to his defence if he should be assailed.

It will also be more in accordance with his own life if little is said about his personal affairs, which took such a very inferior place in his occupations. Unlike many men of the

greatest attainments, Dalton was little occupied with those numerous incidents closely relating to family and friends, which, although productive of much true happiness, capable equally of enlarging the smallest minds and deepening the influence of the most gifted, involve the consumption of much time, a loss much deplored as often as we consider how little we have, and how much is needful in order to obtain from nature even the smallest addition to our knowledge. For Dalton, science was the occupation of life, of a life spent in the most laborious manner. The amenities of life came to him as memories of what had been in his childhood rather than as pleasures realized at the time, memories certainly which he willingly recalled, but as willingly, or perhaps resolutely, left, because his work was before him. He was a student of nature from his cradle.

Few as the materials for his early life are, and bare as are all the narratives, we have perhaps all that could really be found to be interesting. To all appearance he was like those around him, born to be a clodhopper, few things happening to fix the attention of others upon him, no incidents worth a record, because happening to millions daily; the first years must be passed with a simple record of the meagre living and the scantier education he received, until the glow of his life became warmer than that of his fellows. From that time his life is almost entirely in his works, like a devotee who has no heart for the world, but for Divine truth only, his very visits to his early friends were visits as much made for the investigation of truth, made to nature under that aspect which first taught him to observe and to think, which in fact first made him a philosopher and the love of which he never for a moment lost.

DALTON'S EARLY LIFE AND FIRST BOOK.

John Dalton was born at Eaglesfield, in Cumberland, on the 5th of September, 1766, in a small cottage on the estate of

the family, which had come into their possession through his maternal grandmother. His father at this time was a woollen weaver, and did not live in the house belonging to the property, but in a cot of his own, having two small rooms below, one some ten feet square only, and the other still less. The larger house was afterwards, on the death of his uncle, occupied by the father. It is described as one of the better class of farm houses of the district, and is still in possession of the family in a somewhat modernized state. The village of Eaglesfield is 2¾ miles south-west of Cockermouth. The whole township contains only 371 inhabitants. His father, Joseph Dalton, was very poor, in fact he earned only a scanty subsistence by weaving common country goods, and his wife eked it out by selling paper, ink, and quills,* but he seems to have been a man of some vigor of mind, as we find that he taught his sons mathematics, giving them such an education at least as is included in mensuration and navigation.† He afterwards inherited the estate, his brother having died childless. He then gave up weaving. It was by a similar occurrence that the property long afterwards came into the possession of Dr. Dalton, and afterwards into the hands of his cousins on the mother's side.‡ Of the Daltons, or the relatives by the father's side, yeomen or small proprietors in Cumberland, we can find little information; his mother, Deborah Greenup, through whom the property came, connected him with many families in the neighbourhood. She was the third daughter of a family of one son and seven daughters, living at Greenrigg, Caldbeck. The son was a barrister, and practised in London, but having inherited Greenrigg, retired to it, living there to an advanced age, having no children, and leaving the property to his unmarried sister, Ruth. This aunt of Dr. Dalton left the estate of

* Mr. Bewley's Letter. † Mr. Woolley.

‡ This estate was of about sixty acres, but Dalton's elder brother Jonathan increased it considerably by purchase.

Greenrigg to be divided equally between Jonathan and John Dalton and their cousin, George Bewley. The estate was small and in a mountainous district, so that when sold in 1817 or 18, it brought only £750.*

I shall give in a note the names of his aunts the Greenups and their families, as their children the cousins of Dr. Dalton were most affectionately looked on by him during his whole life, as well as kindly and liberally remembered in his will.†

Jonathan Dalton, the grandfather, joined the Society of Friends, and the family continued with that body, a circumstance that no doubt in a considerable degree influenced the habits and character of Dr. Dalton.

Joseph Dalton and Deborah Greenup had three children, Jonathan, John, and Mary. The sons were taught mathematics, partly we are told, by the father, but they were also sent to Mr. Fletcher, the teacher of the school belonging to the Society of Friends. Eaglesfield was early connected with this society, and is said to have first built a meeting house for that body. In Mr. Fletcher's school John remained until twelve years of age, and during that time he must have made great progress, as we find him immediately beginning to teach. He always spoke with great admiration of Mr. Fletcher, who lived until Dalton himself was advanced in life. Indeed we have no reason to think that even in that small

* Mr. Bewley's Letter.

† Eldest daughter of Greenup family married to Samuel Bristo; many descendents, particularly Rachel and Margaret Lickbarrow, of Kendal, to whom Dalton left legacies. (There was a third, Isabella, living at Dalton's death.) Second daughter married to Thomas Bewley, whose only son was George Bewley, of Woodhall, who formerly had the school in Kendal. The grandson, Thomas Bewley, of Bassenthwaite, to whose letter I am indebted for a history of the whole family, has daughters married to Mr. Abbatt, of Liverpool, and Mr. Benson, of Preston, who went in the funeral procession as nearest of kin to Dalton, and who by his will received legacies. The fifth married to Mr. Dickinson, whose descendent's letter I have quoted. The sixth married in the north of Scotland; whilst the seventh is mentioned in the text.

place a teacher of ability could not be found. There are other parts of Cumberland, the parish of Martindale for example, where the science of mathematics was well taught in the days of our grandfathers, but where both scholars and instruction in the most elementary English books could with difficulty be had within these three years. Dalton must certainly have surpassed the other scholars when he began to teach at the age of twelve. The school was kept in the Friends' Meeting House, at Eaglesfield, still a school-room.* We are not told if he succeeded his teacher in this, or where Mr. Fletcher afterwards lived. We may picture to ourselves the struggles of the determined boy, working hard at his father's farm in the summer time, as we are informed he did, and helping also to repair the old farm house, but working with still more determination in winter which afforded him the chief opportunities for study, and when the boys from the various farms congregated to the school, their parents not being able to spare them from their work during the busy season of the year. We can picture the indomitable youth, as an old pupil of his, John Robinson, now living at Eaglesfield, has pictured him, struggling for that authority needed to maintain order, but feeling that there was no struggle needed to shew the superiority of his information. Being as old or older than himself they would not be silenced or commanded, and determined as himself they challenged him into the surrounding grave-yard to fight. It is not said whether he accepted the challenge, but he sometimes took the more dignified mode of locking-up the more refractory, repeating in the school-room that they might learn their tasks whilst he went to his dinner. For this, however, he was sometimes at least the greatest sufferer, as they broke the windows in revenge.†

* Pens and ink were announced as to be sold within.

† From Mr. Dickinson's Letter.

Occupied in teaching and in the work of the farm he laid the foundations of an active mind and raised up a vigorous well-knit frame, which underwent great exertion till an advanced age with little interruption from ill health. Here we see the self-reliance which was strong in him through life; at an age when most persons are mere children, he sought to some extent to rule; and when most persons have scarcely begun to learn soundly, he sought to teach. Here, also, we see that peculiarity of his mind, which did not seek to acquire a great mass of information otherwise than by investigation, and had more pleasure in making use of what it had attained either by conveying it to others, or as a tool for search. These united causes throw some light on his early grasp at independence, as it was not necessity that compelled him to work, nor the want of the means of living which had never failed him.

As we are obliged to arrive at his early character chiefly by inference, we must the more carefully remember, what is more directly told of him, his great diligence. This continued with him through life, and his theory of success was made in the belief that diligence constituted the main difference amongst men engaged in intellectual pursuits. His principal study in early life was mathematics, which he learned with a boy of the name of William Balderstone, receiving assistance from a gentleman in the neighbourhood of the name of Robinson, who fortunately had education as well as property. He and his accomplished wife readily gave their assistance in conducting the studies of Dalton, and his companion who was in their service. These boys filled with emulation solved problems in numbers and in forms, as active minded boys still do over all the country. Balderstone bet Dalton sixpence on a proposition in geometry, but Mr. Robinson objected, and proposed rather that whoever lost should supply the other with candles during the winter. Dalton's answer is generally said to be, "yan might do it,"

for although his parents had got over one of their early difficulties, that of "keeping him out of the dubs," they had not yet taught him to speak other than his native dialect, which he not unfrequently used also in later life, thereby giving pith or humour to his conversation. We scarcely know if the mention of "dubs" means that he was fonder than other children of such things, or whether his love of nature first took its rise in this common although unpromising amusement.

This acquaintance with Mr. Robinson began when Dalton was about ten years old, about the age when he solved a problem, discussed on a hay field among the farm people, whether sixty yards square or sixty square yards are the same. He at first considered them the same, but reflection shewed him the difference. Dalton seldom failed for want of perseverance; he cheered his weary companion who was soon outstripped, and who lived to look on the youth to whom he supplied candles, with the reverence of those who deeply conscious of ignorance imagine in knowledge the most extravagant powers.

Dalton insisted on the importance of diligence, without however considering that the work on which his fame was founded was done comparatively in early life, and that his subsequent unwearied application in no ways tended to elevate his position in science. There is seldom much fame for the idler, but we err greatly when we say a word to dishonour the greatest of all gifts, which cannot be called by a less name than Divine, the eye of genius.

On leaving the boyhood of Dalton we are not called to look on it with surprise, we see in it indications of force, but an equal display is sometimes apparent in less gifted men. We can scarcely look with wonder at the elevation we have seen him attain above his humble fellow-students and pupils. Self-cultivation, too, is a problem now happily so often solved by those who have nothing of true genius, that we

can only look on his acquirements at that time as attainable with the greatest ease by one who had evidently that gift in great force.

After spending about three years teaching in this school he left Eaglesfield. About this time, getting on in life as he thought, he first saw an umbrella in Cockermouth, and bought one, thinking, as he afterwards expressed it, that he was now becoming a gentleman. This happened in 1781, when he became assistant to his cousin, George Bewley, who kept a school in Kendal. His brother Jonathan had been there as assistant for some time previously. In this place twelve years of his life were spent, and here his true education began. He had learned some Latin and Greek, but neither now nor at any time of his life does he seem to have attended much to literature or philology. He is said to have had such an excellent memory that he repeated some of Anacreon's Odes forty years after he had read them, but a few of Anacreon's musical lines seem to be the common property of school boys, who learn them easily from their sound, whilst this knowledge gives no indication whatever of proficiency in the language. A few old Greek books were sold with the rest of his library, partly his own and partly his brother's small stock, at Kendal. The Greek dictionary, a Schrevelius, seems never to have been used. Reading Greek books was no sport for a man who made forty thousand meteorological observations.

In Kendal he became acquainted with Mr. Gough, a man who although blind from infancy, was possessed of high scientific attainments. The mutual assistance rendered is best expressed in Dalton's own words. "For about eight years during my residence in Kendal, we were intimately acquainted; Mr. Gough was as much gratified with imparting his stores of science, as I was in receiving them: my use to him was chiefly in reading, writing, and making calculations and diagrams; and in participating with him in the pleasure

resulting from successful investigations: but as Mr. Gough was above receiving any pecuniary recompense, the balance of advantage was greatly in my favour; and I am glad to have this opportunity of acknowledging it. It was he who first set the example of keeping a meteorological journal at Kendal." Meteor. Observ. and Essays. Preface, 1834.

In an earlier edition Dalton had mentioned Gough as an unknown friend; in the quotation given he does him that justice which in his lifetime he was forbidden to do. Mr. T. T. Wilkinson mentions that Dr. Whewell and several other distinguished wranglers were prepared for their contests by Mr. Gough. He himself wrote no separate work, but many of his scientific memoirs have appeared. As a man he seems to have inspired great respect in all who knew him, and he must have been no common person of whom Wordsworth wrote;

> Methinks I see him how his eyeballs roll'd
> Beneath his ample brow, in darkness pained
> But each instinct with spirit, and the frame
> Of the whole countenance alive with thought,
> Fancy, and understanding; whilst the voice
> Discoursed of natural or moral truth,
> With eloquence and such authentic power,
> That in his presence humbler knowledge stood
> Abashed, and tender pity overawed.
>
> *The Excursion.*

The poet, in a letter to Mr. Samuel Crompton, of Manchester, writes, "Your conjecture concerning that passage is remarkable; Mr. Gough, of Kendal, whom I had the pleasure of knowing, was the person from whom I drew the picture, which was in no respect exaggerated. He was a most extraordinary person, highly gifted, &c. The sadness which the contemplation of blindness always produces, was in Mr. Gough's case tempered by admiration and wonder in the most affecting manner."

During the time that Dalton was enjoying the instruction and advice, as well as the library and scientific apparatus of

Mr. Gough, his cousin George Bewley gave up the school, and the two brothers who had assisted him thus announced their intention of continuing it.

"Jonathan & John Dalton respectfully inform their Friends, and the Public in General, that they intend to continue the school taught by George Bewley, where Youth will be carefully instructed in English, Latin, Greek, & French; also Writing, Arithmetic, Merchant's Accounts, and the Mathematics. The school to be opened on the 28th of March, 1785.

N.B. Youth boarded in the Masters' house on reasonable terms."

Their sister Mary came to keep the house, and their father and mother, then old people, frequently went to see them, walking through on one day over "mountain and slack" a distance of 45 miles.*

George Bewley lent them some money, probably the house or furniture answering to the sum, which was returned the same year; the father lent them seven guineas, "to be paid 9 mo. 29th," which they paid only a week behind time. And so they began life on a larger scale. They took care of their money, balanced their books every month, and put down every penny they spent. They had more than once to get a guinea from Mr. Lickbarrow and two from Mr. Kendal, and Mary had to give up her thirteen shillings and sixpence, and got paid in portions, "Mary, in part, 0. 0. 6$^{d.}$" Mr. Benson too was paid all his money, and borrowings soon ceased. The whole sum got the first year was about £107, but a good deal of this had to be paid back, and indeed the average of the school was about seventy pounds a year. This was increased a few pounds by "drawing conditions," "collecting rents," "making wills," and "searching registers," but the amount gained by this means was seldom above five pounds a

* John Robinson, in Mr. Dickinson's Letter.

year, although it rises to more than twice that sum in certain years. We do not know if Dalton so employed himself, certainly however his brother did so.

A second circular issued by the Daltons in the following year shews more fully the intentions of the school, and gives us also an idea of the enlarged views of education which Dalton took.

Kendal, July 5th, 1786.

"Jonathan & John Dalton take this method of returning their acknowledgments to their friends and the public for the encouragement they have received since their opening school; and from their care and assiduity in the management of it manifested in the improvement of the Youth under their care, are induced to hope for a continuation of their favours. They continue to teach on reasonable terms English, Latin, Greek, and French;

Also,

Writing,	Mensuration,	Projections,
Arithmetic,	Surveying,	Dialling,
Merchant's Accounts,	Gauging,	Optics,
Geometry,	Algebra,	Mechanics,
Trigonometry,	Fluxions,	Pneumatics,
Navigation,	Conic Sections,	Hydrostatics,
Geography,	Astronomy,	Hydraulics, &c.

N.B. Persons desirous of being instructed in the use of the globes, &c., will be waited upon any time out of school hours. The Public may also be informed, that they could conveniently teach a considerable number more than at present. Those who send their children may depend upon their being carefully instructed."

To his usual instruction in school he added lectures. It is certainly interesting to look over the syllabus published. We see him as an ardent young man, filled with ideas from every science, eager to tell them to others. We must remember

that he is a man of simple mind, who had never seen a large city, to whom the parade of the world and the unfortunate pride of successful science was known only from books, and we need not wonder that he expected on announcing the truths he had learned to an ignorant rural population, a crowd eager to hear them. Independent investigation had not yet contracted his field of interest, so to speak, for we find this effect produced of necessity in the mind which has for a long time travelled alone over an unmapped district. He is now 21 years of age, and gives the following programme of lectures.

Oct. 26th, 1787.

"Twelve lectures on Natural Philosophy to be read at the school (if a sufficient number of subscribers are procured) by John Dalton. To begin on Tuesday evening the 13th Nov$^{r.}$ next, at 6 o'cl., and to continue every Tuesday and Thursday at the same hour till compleated.

Subscribers to the whole ½ a guinea; or one shilling for single nights.

N.B. Subscribers to the whole course will have the liberty of requiring further explanation of subjects that may not be sufficiently discussed or clearly perceived when under immediate consideration; also of proposing doubts, objections, etc.; all which will be illustrated and obviated at suitable times to be mentioned at the commencement.

A Syllabus of the Lectures.

First & Second. Mechanics.

Introduction. Rules of Philosophizing on Matter and its Properties with the different opinions of the most famous Philosophers on this head.

The laws of motion. Mechanic powers. Vibration of pendulums.

Third, Fourth, & Fifth. Optics.

Preliminary discourse. Of the nature and properties of light. Of simple vision. Doctrine of colours. Of reflected

vision. Of mirrors and images reflected from them. Of refracted vision, with the nature of lenses and images exhibited thereby. Of burning glasses. Description of the eye. Manner of vision. Of long and short sighted eyes. Of spectacles, telescopes, & microscopes. Of the rainbow.

Sixth & Seventh. Pneumatics.

Of the atmosphere. The elasticity of the air. Description of the air pump. The spring and weight of the air proved by a great variety of experiments on the air pump. Of respiration. Of sound. Of winds. Of the blueness of the sky. Of twilight.

Eighth, Ninth, & Tenth. Astronomy.

Introduction. Of the solar system. Of the figures, magnitudes, distances, motion, &c. of the sun, planets, and comets. Of the progressive motion of light. Of the fixed stars and their phenomena. Of the lunar planets. Of eclipses, tides, &c.

Eleventh & Twelfth. Use of the Globes.

Figure of the Earth. Description of the globes. Various problems performed thereon, amongst which are, an explanation of the phenomena of the harvest-moon and the variations of the seasons. Conclusion.

'Ex rerum causis supremam noscere causam.'"

Miss Johns, whose diary will be spoken of below, tells us that this very syllabus and one for 1792 came accidentally in his way in after life when he was looking over some old letters, having been detained in the house by a cold. He burst out into a loud laugh.

The accounts given of the Daltons as teachers lead us to believe them to have kept up the old system of great sternness and formality, although John's character seems to have been the milder of the two. Even during school hours he was much occupied with mathematics and making calculations at all spare moments on bits of paper that came in

his way. This sternness of manner never left him, although his disposition was undoubtedly gentle. It may have arisen from having been compelled so long to continue his pursuits without the companionship of congenial minds, although even that does not sufficiently explain the cause, as we hear of others less befriended than Dalton whose disposition has retained its vivacity. It is greatly to be regretted that a journal containing all the minute particulars of his life at this period should be entirely lost. Portions which have been read to me by his friend Peter Clare introduce us into his character in a very pleasing manner. We find him cheerful and easy, fond of a little innocent sport, and much attached to some games, but still so precise that every one was rigidly recorded and the results of the play of each party systematically compared. One evening at a house he visited, the company spent their time making verses; when the last word of one verse was told, the next person in order was expected to make a line to rhyme to it. It is curious to observe that every couplet, as well as the author of each, is carefully noted down in the diary. In this we have an early illustration of the great order that was a prominent point to be remembered in judging of his intellectual character. This does not deny the paradoxical addition of great carelessness. It was at this time that he was more especially a student of the Lady's Diary, and one of those who solved its problems, obtaining on several occasions the prize. Even at Eaglesfield, however, he was employed, although not with equal success, in the same manner, as Mr. Dickinson says, "When I was a boy I saw John Dalton at my cousin William Alderson's house, in Eaglesfield; they talked of days when they were lads together, sitting over the fire till midnight poring over the Lady's Diary. John never giving sleep to his eyelids until he had found out the riddle of some prize enigma or some mathematical question."

We find him at this time making his own barometers and

thermometers, making experiments on hygrometers of whip cord, sending specimens of butterflies to Mr. Crosthwaite's museum in Keswick, and afterwards engaging for a very small sum to send him dried specimens of plants. Mr. T. P. Heywood, of the Isle of Man, has in his possession eleven volumes of his *Hortus Siccus*. The first is a thick volume, containing the general title page, *Hortus Siccus seu Plantaram diversarum in Agris Kendal vicinis sponte nascentium Specimina, Opere et Studio Johannis Dalton collecta, et Secundum classes et ordines disposita.* 1790." The other volumes are thin. They are not preserved with the greatest care which collectors are capable of, but they are a proof of great industry and of considerable attainment, even in a branch of science which he did not profess. He supplied Mr. Crosthwaite with a barometer and thermometer, although not knowing how to make them. He nevertheless begins and learns their faults by experience. A letter given by Dr. Henry shews us how his knowledge stood at this time, and how also he was in the habit of acquiring it. Speaking of the mercury in the barometer, he says, "I intend to renew mine as soon as convenient; if thou do the same, be careful in undoing it; and attend to the cautions I give. Be sure to rub the inside of the tube well with warm dry cotton or wool, and have the mercury when poured in at least milk warm, for moisture is above all things else to be avoided, as it depresses the mercury far more than a particle of air does; mine is, as I have said, at least $\frac{1}{16}$th of an inch too low, and yet it is clear of air, and to all appearance dry; but I doubt not but attending to these precautions, which I knew nothing of when it was filled, will raise it up to its proper height."

At this time he felt uneasy, the sphere of his simple school was too small, his impatience took the form of variety in his pursuits, and he wandered over nearly every branch of science. He seems now, although at a later time than generally happens to young men, to have been "yearning

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for that large" existence which he knew to be somehow attainable. He thought of a profession. His uncle Thomas Greenup, the barrister, thought that it was entirely out of the reach of a person in Dalton's circumstances to be a barrister or a physician, and recommended rather as less difficult, but still much above him, that of an attorney or apothecary. It was at this time that he began to make medical experiments, wishing to ascertain the loss from the human body by insensible perspiration, a sufficient proof that however much he might have advanced the knowledge of that profession, he was too much an experimenter and solitary thinker to have been pleased with the active life of some of our medical men. The discouragement received from his friends seems to have prevented all exertion in the new direction he had contemplated, and he remained three years longer or until 1793 in Kendal, when Dr. Barnes asked Mr. Gough for a suitable person to teach mathematics in the New College of Manchester. This college had arisen out of the Warrington academy, where Priestley had taught, as well as Dr. Aikin, Dr. Enfield, Reinhold Forster, and Gilbert Wakefield. At that time the college was in the present "College-buildings," in Mosley-street. He lived in the establishment, and remained tutor of mathematics and natural philosophy for six years. Dr. Barnes was the principal. This college is now transferred to Gordon-square, London. Whilst here we find from papers lent me by Mr. Woolley, that in 1794 he had twenty-four pupils for mathematics, mechanics, geometry, algebra, book-keeping, natural philosophy, and chemistry. He used Lavoisier's Elements of Chemistry and Chaptal's amongst others. In 1799 there were twenty-two students. Although Manchester is now multiplied by four, it cannot shew the same number, and I fear that the love of external things has overpowered the love of science.

As soon as he gave up the intention of studying for a profession, he seems to have decided at once on a regular course

of investigation and in the interval from that time to 1793, when he went to Manchester, produced his first work properly so called, his "Meteorological Observations and Essays;" they were not, however, published until he had taken up his position at the college.

This book contains an extensive series of observations on old and new subjects, comprising ideas sometimes new, sometimes old, and at other times modifications of the old. He enters into the discussion of the cause of the rise and fall of the barometer, which he decides to be the existence of the vapour of water in the air. Also he discusses the state of water in the atmosphere, shewing it to be an elastic vapour existing like any other gas not in chemical combination. Then he treats of evaporation from the earth's surface, clouds and rain, and allied phenomena, bringing, as Professor Sedgwick says, "the elements themselves under his own intellectual domination." The extent and variety of these inquiries prove the earnestness with which he studied in his almost solitude in Cumberland. The work seems to have been at first intended as a popular treatise on meteorology. It begins with a description of the barometer, then come the thermometer, hygrometer, and rain gauges; connected with these are tables of observations made at Kendal and Keswick. There seems to be a looseness of description in the first part of the volume, which would seem to imply that the matter was easily understood, and the readers could make out the particulars for themselves. As he proceeds, however, he seems to feel that he has a harder task to perform, and speaks rather to scientific than to popular hearers, whilst we gradually become aware that he is a close and precise reasoner. His style is very simple; he goes directly to his point; all inessential parts are left out. He seems to move forward with a heavy dogged tread, never turning his head aside, but as any style may become a fault if too far carried out, we find that in his there are left out many things that

are certainly needful as accessory or confirmatory, leaving what to the eyes of many is a want of finish, so that others have been needed to complete what was in reality sufficiently complete had it been laid out entire as it existed in his own mind.

As an example of his style, at p. 97, we find

"It appears from the observations (see table p. 15) that the mean state of the barometer is rather lower than higher in winter than in summer, though a stratum of air on the earth's surface always weighs more in the former season than in the latter; from which facts we must unavoidably infer that the height of the atmosphere, or at least of the gross parts of it, is less in winter than in summer, conformable to the table p. 80. There are more reasons than one to conclude that the annual variation in the height of the atmosphere, over the temperate and frigid zones, is gradual, and depends in a great measure on the mean temperature at the earth's surface below, for clouds are never observed to be above four or five miles high, on which account the clear air above can receive little or no heat, but from the subjacent regions of the atmosphere, which we know are influenced by the mean temperature at the earth's surface; also, in this respect, the change of temperature in the upper parts of the atmosphere must in some degree be conformable to that of the earth below, which we find by experience increases and decreases gradually each year, at any moderate depth, according to the temperature of the seasons.

"Now with respect to the fluctuations of the barometer, which are sometimes very great in twenty-four hours, and often from one extreme to the other in a week or ten days, it must be concluded, either that the height of the atmosphere over any country varies according to the barometer, or otherwise that the height is little affected therewith, and that the whole or greatest part of the variation is occasioned by a change in the density of the lower regions of the air. It is

very improbable that the height of the atmosphere should be subject to such fluctuations, or that it should be regulated in any other manner than by the weekly or monthly mean temperature of the lower regions; because the mean weight of the air is so nearly the same in all the seasons of the year, which could not be if the atmosphere was as high and dense above the summits of the mountains in winter, as it is in summer. However, the decision of this question need not rest on probability; there are facts which sufficiently prove, that the fluctuation of density in the lower regions has the chief effect on the barometer, and that the higher regions are not subject to proportionable mutations in density. In the Memoirs of the Royal Academy at Paris, for 1709, there is a comparison of observations upon the barometer, at different places, and amongst others, at Zurich, in Switzerland, in lat. 47° N., and at Marseilles, in France, lat. 43° 15′ N.; the former place is more than 400 yards above the level of the sea; it was found that the annual range of the barometer was the same at each place, viz., about 10 lines; whilst at Genoa, in latitude 44° 25′ N., the annual range was 12 lines, or 1 inch; and at Paris, latitude 48° 50′ N., it was about 1 inch 4 lines. In the same memoir it is related that *F. Laval* made observations, for ten days together, upon the top of St. Pilon, a mountain near Marseilles, which was 960 yards high, and found that when the barometer varied $2\frac{1}{4}$ lines at Marseilles, it varied but $1\frac{3}{4}$ upon St. Pilon. Now had it been a law, that the whole atmosphere rises and falls with the barometer, the fluctuations in any elevated barometer would be to those of another barometer below it, nearly as the absolute heights of the mercurial columns in each, which in these instances were far from being so. Hence then it may be inferred, that the fluctuations of the barometer are occasioned chiefly by a variation in the density of the lower regions of the air, and not by an alternate elevation and depression of the whole superincumbent atmosphere. How we conceive this fluc-

tuation in the density of the air to be effected, and in what manner the preceding general facts relative to the variation of the barometer may be accounted for, is what we shall now attempt to explain." This is referred to the varying amount of vapour.

In section 5, "observations on the height of clouds," there is given the summary of 5381 observations, made by Mr. Crosthwaite, an evidence of the intellectual diligence to be found at the lakes even before they became the haunt of poets.

At p. 127 he gives a table of the temperature of water made to boil at different atmospheric pressures, bearing on the fact, which he is there explaining, that "aqueous vapour always exists as a fluid sui generis diffused amongst the rest of the aerial fluids;" and at p. 129, "that it may be determined a priori what weight of vapour a given bulk of dry air will admit of, for any temperature, provided the spec. grav. of the vapour be given." These conclusions appear more fully in a note* to a paper read in 1797, after having made confirmatory experiments. This must be taken as an elucidation of a subject which afforded much discussion at that period. We know that Saussure and others knew well that moisture existed in the air at very low temperatures, and there was a variety of opinions as to the state in which it existed. Many writers of the period believed, that because warm air was sensibly drier, it contained less moisture than cold air; all these points Dalton has elucidated and spoken on with decision.

In the third and sixth essays, the precise point where the burden of his argument is contained is difficult to find, the reasoning being a constant process to prove what it is supposed we knew beforehand to be the result. In the appendix, he says more pithily, p. 188, "I am confirmed in the opinion that *the vapour of water, and probably of most other liquids, exists at all times in the atmosphere, and is capable of bearing any known degree of cold without a total condensation, and that*

* Memoirs of the Manchester Philosophical Society. Vol. V., p. 351.

the vapour so existing is one and the same thing with steam, a vapour of the temperature of 212° *or upwards.* The idea, therefore, that vapour cannot exist in the open atmosphere under the temperature of 212°, unless chemically combined therewith, I consider as erroneous; it has taken its rise from a supposition, that air pressing upon vapour condenses the vapour equally with vapour pressing upon vapour, a supposition we have no right to assume, and which I apprehend will plainly appear to be contradictory to reason and unwarranted by facts; for, when a particle of vapour exists between two particles of air, let their equal and opposite pressures upon it be what they may, they cannot bring it nearer to another particle of vapour, without which no condensation of vapour can take place, all other circumstances being the same; and it has never been proved that the vapour in a receiver from which the air has been exhausted, is precipitated upon the admission of perfectly dry air. Hence, then, we ought to conclude, till the contrary can be proved, *that the condensation of vapour exposed to the common air does not in any manner depend upon the pressure of the air.*"

At p. 135, after contending for the theory that the vapour of water is mixed with the air and not combined, he explains how it is precipitated by cold and taken up by heat, and how it is that clouds consisting of light drops do not fall so readily as clouds with heavy drops, as the resistance of the drops is as the square of the diameter, in which his mathematical knowledge helps his meteorology. This was suggested by Mr. Gough.

There is in these Essays, and everywhere in Dalton's writings, a great rapidity of reasoning, a direct passage from premise to conclusion without fear, as if more than usually persuaded that true reason could not misguide him, so that he is utterly regardless of consequences.

At p. 168 we find a fair example of his mode of reasoning, and one also of his daring theories.

"The light of the aurora has been accounted for on three or more different suppositions:—1. It has been supposed to be a flame arising from a chymical effervescence of combustible exhalations from the earth. 2. It has been supposed to be inflammable air, fired by electricity. 3. It has been supposed electric light itself."

"The first of these suppositions I pass by as utterly inadequate to account for the phenomena. The second is pressed with a great difficulty how to account for the existence of *strata* of inflammable air in the atmosphere, since it appears that the different elastic fluids intimately mix with each other; and even admitting the existence of these *strata*, it seems unnecessary to introduce them in the case, because we know that discharges of the electric fluid in the atmosphere do exhibit light, from the phenomenon of lightning. From these and other reasons, some of which shall be mentioned hereafter, I consider it almost beyond doubt that the *light* of the *aurora borealis*, as well as that of falling stars and the larger meteors, is electric light solely, and that there is nothing of combustion in any of these phenomena. Air, and all elastic fluids, are reckoned amongst the non-conductors of electricity. There seems, however, a difference amongst them in this respect: dry air is known to conduct worse than moist air, or air saturated with vapour. Thunder usually takes place in summer, and at such times as the air is highly charged with vapour; when it happens in winter, the barometer is low, and consequently, according to our theory of the variation of the barometer, there is then much vapourized air; from all which it seems probable, that air highly vapourized becomes an imperfect conductor, and, of course, a discharge made along a stratum of it will exhibit light, which I suppose to be the general cause of thunder and lightning."

"Now, from the conclusions in the preceding sections, we are under the necessity of considering the *beams* of the *aurora borealis* of a *ferruginous* nature, because nothing else is

known to be magnetic, and consequently, that there exists in the higher regions of the atmosphere an elastic fluid partaking of the properties of *iron*, or rather of *magnetic steel*, and that this fluid, doubtless from its magnetic property, assumes the form of cylindric beams. It should seem, too, that the rainbow-like arches are a sort of *rings* of the same fluid, which encompass the earth's northern magnetic pole, like as the parallels of latitude do the other poles; and that the beams are arrayed in equidistant rows round the same pole. * * * Things being thus stated, I moreover suppose that this elastic fluid of magnetic matter is, like vapourized air, an imperfect conductor of electricity; and that when the equilibrium of electricity in the higher regions of the atmosphere is disturbed, I conceive that it takes these beams and rings as conductors, and runs along from one quarter of the heavens to another, exhibiting all the phenomena of the *aurora borealis*."*

In the edition of 1834 he still adheres to the same theory; some will look on it as absurd; it is certainly the result of great daring, or in other words, it may be viewed as the reasoning of a man who has exhausted all his knowledge in finding a cause, feels certain that there is one, and decides upon that which is most conformable to his knowledge, without waiting for a wider view, or for a time when something perfectly new might entirely change the scene.

This essay on the aurora he considered as of great importance. He begins with these words; "As this essay contains an original discovery which seems to open a new field of inquiry in philosophy, or rather, perhaps, to extend the bounds of one that has been as yet but just opened; it may not, perhaps, be unacceptable to many readers to state briefly the train of circumstances which led the author to the important conclusions contained in the following pages." And yet we take up treatises on the aurora, and do not even find Dalton's name

* The pages refer to the new edition.

mentioned. Wargentin, Halley, and Celsius had all observed the action of the aurora on the magnetic needle. Dr. Halley had supposed it to be caused by magnetism. Dalton went more fully into the subject than his predecessors, without, however, taking all difficulty from it. In this treatise we see an instance of the pertinacity with which he held ideas which he had formed. But we find him altering his opinion on the height of the aurora; his observations led him to believe the height to be about 150 miles; afterwards he considered it to be about 100. Numerous as have been the attempts to ascertain the height, the differences ranging from feet to thousands of miles, Dalton still, in 1834, severely criticised all the observations which differed greatly from his early results. To this treatise on meteorology he added little, although a new edition appeared after forty years. He then says, that it is printed verbatim (adding only a small appendix), "as I apprehend it contains the germ of most of the ideas which I have since expanded more at large in different essays, and which have been considered as discoveries of some importance." But he says also, that "the subject here treated of appeared to the author to be very imperfectly appreciated, or little understood, by some of the modern writers on meteorology," and it is probably true that the facts and theories he advanced had, in some or many instances, been worked out by others with little aid from his book, because, although occasionally quoted, it was really very little known. This arose from a peculiarity in his mode of publishing it. It was like all his books printed for himself, and was never allowed to make its due way in an independent manner among the booksellers, nor had the essays the advantage of being read to a society, or given out by any journal.

CHAPTER II.

HIS EPOCH OF GREATEST FERTILITY.

THE work on meteorology was published in September, 1793. Dalton had come to Manchester in the spring of that year. He was now twenty-seven years of age. The removal to an active town seems to have satisfied his cravings for a larger sphere of labour which were forcing him from his attachment to his neighbourhood. He was self-taught, a raw countryman, in many respects rather rough in his acquired habits, although of a naturally gentle disposition. Such a distance from active life would have made many men idle, such a sudden entrance into it has often the same effect on others. Neither seemed to affect him, there was little change of habit, he was still in the streets of Manchester as on the hills of Cumberland, the active observer and thinker. On October 3rd, 1794, he first appears as a member of the Literary and Philosophical Society of Manchester, having been proposed by Thomas Henry, Dr. Percival, and Robert Owen, the veteran enthusiast who would willingly compel all mankind to be reformed by his simple formula. On the 31st, he read his first paper to the society, an event to him of great importance, greatly influencing all his future life, as he soon after became the representative of that body, continuing so for the remainder of his life.

This paper was entitled "Extraordinary Facts relating to the Vision of Colours."* He says there, p. 30,

"It may be proper to observe, that I am shortsighted. Concave glasses of about five inches focus suit me best. I can see distinctly at a proper distance; and am seldom hurt by too much or too little light; nor yet with long application."

* Memoirs of the Philosophical Society of Manchester. Vol. V., p. 28.

"I found that persons in general distinguish six kinds of colour in the solar image; namely, red, orange, yellow, green, blue, and purple. To me it is quite otherwise; I see only two, or at most three, distinctions; these I should call yellow and blue, or yellow, blue, and purple. * * * * My yellow comprehends the red, orange, yellow, and green of others, and my blue and purple coincide with theirs."

He sums up the peculiarities of the vision of himself and others who have been found similarily affected thus; p. 40.

"1. In the solar spectrum three colours appear—yellow, blue, and purple. The two former make a contrast; the two latter seem to differ more in degree than in kind.

2. *Pink* appears, by day light, to be sky-blue a little faded; by candle light it assumes an orange or yellowish appearance, which forms a strong contrast to blue.

3. *Crimson* appears a muddy blue by day; and crimson woollen yarn is much the same as dark blue.

4. *Red and scarlet* have a more vivid and flaming appearance by candle light than by day light.

5. There is not much difference in colour between a stick of red sealing wax and grass, by day.

6. Dark green woollen cloth seems a muddy red, much darker than grass, and of a very different colour.

7. The colour of a florid complexion is dusky blue.

8. Coats, gowns, &c., appear to us frequently to be badly matched with linings, when others say they are not. On the other hand, we should match crimsons with claret or mud; pinks with light blues; browns with reds; and drabs with greens.

9. In all points where we differ from other persons, the difference is much less by candle light than by day light."

He found several persons having the same peculiarity of vision, and says (p. 43), "It appears, therefore, almost beyond a doubt, that one of the humours of my eye, and of

the eyes of my fellows, is a coloured medium, probably some modification of blue."*

Although this paper was an observation on himself, it is in reality a discovery; the facts had not been arranged before he arranged them, and found out other persons similarly situated. A peculiar keenness of reasoning was needed to find it out, as we must remember that with such persons there is little community of feeling on colour, and scarcely a mode of judging whether they see any colours exactly as the normal eye does. It would probably explain many strange occurrences if we were to consider that there are really persons in the world who see all crimsons as "dark blue" or "a muddy blue," and who would "match crimsons with claret or mud; pinks with light blues; browns with reds; and drabs with greens;" who see the healthful tints of a florid complexion to be like "dilute black ink on white paper," or "a dull opake blackish blue, upon a white ground." How many strange mistakes and visions might be accounted for by this defect of sight. A fair face with glowing veins would be to Dalton as a corrupting corpse. But it may be said that custom would make all appear as well to him as to others; no, it cannot be so: a defect must constantly carry with it the consequences of a defect, and in this case the established difference which nature has made between life and death, beauty and horror, was hidden from the eye, and therefore to a great extent must have been concealed from the intellect. To this cause partly we may refer that want of fine sensibility to external things which peculiarly marked his scientific as well as social life.

Dr. Whewell has called such persons *idiopts*, because their vision is peculiar; this is not sufficiently characteristic, and

* Mr. J. A. Ransome, who examined the eye after death, found nothing whatever to account for the peculiarity of vision. Certainly colours appeared as usual through it. He believed that the cause was a deficient sensorial or receptive power.

as has been remarked sounds badly, Sir John Herschel having changed it to Dichromic vision, believing that one of the three colours is lost to the eye entirely. Such a vision there seems to be, but this extent has not been observed in any instances, by Dr. George Wilson, who thinks that there is no colour quite lost, although the power of perceiving be feeble, and he names it *Chromato-Pseudopsis*, or a false vision of colours. This he has translated by Sir D. Brewster's term, colour-blindness, which appears much too strong when we consider that some colours are well seen, and others seen in part. It seems, in fact, to be an imperfection in the power of distinguishing colours, which may exist to any extent, either very slightly, as is seen in every-day life, where, for example, among the many workpeople in a large mill, only a few are found fit for arranging yarns with accuracy. A nice perception of colour is there a valuable gift, and is paid for accordingly. Or it may occur decidedly defective, as with Dr. Dalton and others. Dalton's brother had the same defect, and one or two others in the neighbourhood of Eaglesfield, of whom I have lately heard. It is probable that there are many gradations, beginning with deficient colour sight and ending in Dichromic, or perhaps Monochromic or Achromic vision, or true colour blindness. Dr. Wilson well remarks, that Daltonism, under which it has been known, is not a proper name for the peculiarity, as it connects his name with a defect. Indeed few eyes are found equal to Dalton's, if we judge of them by their results. Dr. Wilson has made the remarkable discovery that this defect may almost be called common.

Dalton remained without giving anything to the public until 1799. In the College his order showed itself in the careful list of students and their lessons, still remaining. Possibly his duties occupied too much of his time to allow of experiment, but he comes out so suddenly after that as physikist and chemist, that his time must have been spent

in suitable studies. On March 1st, 1799, he read to the Literary and Philosophical Society* "Experiments and observations to determine whether the quantity of rain and dew is equal to the quantity of water carried off by the rivers and raised by evaporation; with an inquiry into the origin of springs." In this he treats,—

"1. Of the quantity of rain and dew.
2. Of the quantity of water that flows into the sea.
3. Of the quantity of water raised by evaporation.
4. Of the origin of springs."

The first three are accompanied by experiments, but there is a looseness in the calculations which renders the paper rather like a sketch of the subject. He, however, collects a great deal of information as to the annual fall of rain in various places, and in a note explains clearly, as before alluded to, his ideas as to the state of aqueous vapour in the air. The looseness of expression is not at all times with him an indication of want of decision, but his peculiar style of writing, as if every one knew the subject, and were ready to draw out his reasoning into all its details, as soon as expressed. His experiments, begun with the hand, seem often finished with the head, so rapidly are his conclusions come to, and the natural law established in his mind. Even now we can add little to the relation between evaporation, rain, and dew, and on the origin of springs he is clear, quick, and decisive, saying that they come from the rain. This subject had been much disputed; filtration from the sea having been a favourite method of obtaining the water, as well as subterranean reservoirs like those of Father Kircher, who shows them in engravings continually boiling out from the centre of the earth. Dalton was not the first to suggest the explanation, of course, but the subject was sufficiently uncertain to call for elucidation. On April 12th,

* Memoirs of the Literary and Philosophical Society of Manchester. Vol. V., p. 346.

1799, he read a paper entitled "Experiments and observations on the power of fluids to conduct heat, with reference to Count Rumford's seventh essay on the same subject."*

He seems evidently to have made up his mind at once that Count Rumford had drawn a wrong conclusion from his premises, and we see in the reasoning much minute ingenuity and acuteness. As an example of his mode of experimenting and reasoning, the following may be given, p. 381.

"Exp. 3. Took an ale glass of a conical figure, 2½ inches in diameter, and 3 inches deep; filled it with water that had been standing in the room, and consequently of the temperature of the air nearly. Put the bulb of a thermometer to the bottom of the glass, the scale being out of the water; then having marked the temperature, I put the red-hot tip of a poker half-an-inch deep into the water, holding it there steadily about half-a-minute; and as soon as it was withdrawn, I dipped the bulb of a sensible thermometer about ½ inch, when it rose in a few seconds to 180.°

TEMPERATURE.

Time.	At Top.	Middle.	Bottom.
Before the poker was immersed		...	47°
—	180°	—	47°
5 min.	100°	60°	47½°
20 ,,	70°	60°	49°
1 hour	55°	—	52°"

After other experiments he says, p. 385, "We must conclude, therefore, that the quick circulation of heat in water over a fire, &c., is owing *principally* to the internal motion excited by an alteration of specific gravity; but not *solely* to that cause, as Count Rumford has inferred."

A very simple and ingenious experiment is related on the same page. He mixed hot and cold water, stirred for half-a-minute, and tried if the upper part became hotter than the lower, it

* Same vol., p. 373.

did not do so, on which he says, "If the particles of water during the agitation had not actually communicated their heat, the hot ones ought to have risen to the top, and the cold ones subsided, so as to have made a material difference in the temperature." This shows, that even at that period he was accustomed to think habitually of matter as decidedly atomic in its constitution.

On the theoretical conclusion to be drawn here, we find his genius taking the lead; he is accurate in spite of the rudeness of his experiments. He concludes that water conducts heat a little, and that the expansion of water is the same both above and below the point of maximum density. But when he comes to determine the precise place at which that point is found, as it is a matter of experiment, and cannot be got by the mind only, he is at fault; in subsequent experiments learning to become accurate.

He seems to have lowered the point to 36°, and afterwards considered it 38°, the point now apparently fixed on is 39°, or 39.101. (Playfair and Joule.) Dr. Hope's experiments gave it as between 39½ and 40 degrees. In this investigation Dalton's mind again analyses itself, dividing to great clearness of conception on the one side, and carelessness of minute observation on the other.

In 1830 on reading over some old letters which he was arranging, he found one from Dr. Hope, saying, "notwithstanding the caution you gave me, I venture to publish my pamphlet on the contraction of water by heat," Dr. Dalton said, "aye, he had the advantage of me there, but not so much as it appeared at first sight."

In this paper he makes an observation on the power of capillary tubes to prevent the freezing of water, a circumstance which has not been thoroughly inquired into, nor the cause assigned its proper place.

In May, 1800, Dalton was elected secretary of the Literary and Philosophical Society of Manchester, in the

place of Dr. William Henry, and having as his colleague Dr. Hull. This office he retained until the year 1808, when he was made vice-president in the room of Dr. Roget, who then lived in Manchester. Soon after, on June 27th, he read to that Society, "Experiments and observations on the heat and cold produced by the mechanical condensation and rarefaction of air." * Here by well devised experiments he endeavoured to shew what however had been before held by Lambert, Saussure, and Pictet, "that the capacity of a vacuum for heat is less than an equal volume of atmospheric air, and that the denser air is, the less is its capacity for heat," indicating a mode of ascertaining "the absolute capacity of a vacuum for heat," and "likewise the capacity of the different gases for heat by a method wholly new." An important result of these experiments was, that the temperature of air mechanically compressed to one-half its volume was raised 50°. This, although much underrated, was the first numerical result of importance on this subject. p. 524.

In this paper we find that he had ascertained that gases expand 1-10th of their volume nearly for 50° of heat, or nearly 1-500th of their bulk, a subject which he treated of at a later period.

Whilst engaged in teaching at the academy in Manchester, his classes or scholars seem to have been as miscellaneous as they had been at Kendal. We may infer this from the appearance of an English grammar, the preface of which is dated March 10th, 1801. He seems to have looked on this as a recreation, but he never afterwards appears to have had recourse to literature for amusement or for variety. As he has never been looked on as a grammarian, it may be of some interest to see what his views on such points were.

He says, p. 8. "It may be taken as an axiom that all time or duration in the strict sense of the terms, is either

* Mem., Vol. V., p. 515.

past or *future*. But for the purposes of speech we must have a present time of some duration, which must necessarily be comprised of a portion of past and a portion of future, having the present, *now* or *instant*, as a boundary between them. Its length may be what we please to make it.

" Grammatically speaking therefore, there are three times, present, past, and future; though strictly and mathematically speaking, we can admit only two, past and future. Moreover we find it expedient in the course of conversation, not only to mention actions as whole and entire, but also their commencement, their being in a passing or middle state, and their termination; accordingly our language furnishes us with four forms of speech for each of the times or tenses, which are exemplified in their proper place, both for the active and passive verb, with appropriate names to them."

His active verb is given thus:—

Indicative mood.

Present time.

I serve, &c.

Beginning present.

I am about to serve, &c.

Middle present.

I am serving, &c.

Ending present.

I have served or been serving, &c.

Past time.

I served, &c.

Beginning past.

I was about to serve, &c.

Middle past.

I was serving, &c.

Ending past.

I had served or been serving, &c.

Future or present time.

I shall, will, may, can *or* must serve, &c.

Beginning future or present.
I shall, will, may, can *or* must be about to serve, &c.
Middle future or present.
I shall, will, may, can *or* must be serving, &c.
Ending future or present.
I shall, will, may, can, *or* must have served *or* have been serving.

In grammar it is difficult to have absolutely new ideas, the subject has been so belaboured, and at the same time it is not easy to keep rigidly to any system proposed, so many of the treatises have wanted clearness. We may see that in that department Dalton was inclined to be an innovator, although he has not the honor of being a discoverer, indeed his mind was much too rigid to be inclined to yield to all the flexions and variations of a subject so bordering on metaphysics as grammar. Horne Tooke is the writer which he most admired on that subject, using sometimes his very words, although not in all things following him. But innovators are more dangerous in grammar, and are less easily received, than in the physical sciences which have no ancestry.

Some years afterwards he went into the shop of the publisher of his grammar, and asked for a copy; he was told they had none, but insisting on it, a parcel of them was found in some dusty corner, very few having ever been sold. Still he assures us that a Sheffield man had published it some years later as his own, with some additions.

In October of the same year he read a paper which occupied three evenings of the Literary and Philosophical Society. It is composed of four "Experimental essays on the constitution of mixed gases; on the force of steam or vapour from water and other liquids in different temperatures, both in a Torricellian vacuum and in air; on evaporation; and on the expansion of gases by heat." (Mem. Vol. V., p. 535.)

The four laws given by him are—

"1. When two elastic fluids, denoted by *A* and *B*, are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of *A* do not repel those of *B*, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

"2. The force of steam from all liquids is the same, at equal distances above or below the several temperatures at which they boil in the open air; and that force is the same under any pressure of another elastic fluid as it is in vacuo. Thus the force of *aqueous* vapour of 212° is equal to 30 inches of mercury; at 30° below, or 182°, it is of half that force; and at 40° above, or 252°, it is of double the force; so likewise the vapour from sulphuric ether, which boils at 102°, then supporting 30 inches of mercury, at 30° below that temperature it has half the force, and at 40° above it, double the force; and so in other liquids. Moreover the force of aqueous vapour of 60° is nearly equal to ½ inch of mercury, when admitted into a Torricellian vacuum; and water of the same temperature, confined with perfectly dry air, increases the elasticity to just the same amount.

"3. The quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same.

"4. All elastic fluids expand the same quantity by heat; and this expansion is very nearly in the same equable way as that of mercury; at least from 32° to 212°. It seems probable the expansion of each particle of the same fluid, or its sphere of influence, is directly as the quantity of heat combined with it; and consequently the expansion of the fluid as the cube of the temperature, reckoned from the point of total privation."

The first law accounts for a diffusion of gases to a great extent, but not entirely. It would result from it, if not qua-

lified, that there would be a diminishing quantity of oxygen, which is the heaviest gas in the atmosphere, according as the height increased. This was Dalton's opinion, but it has not turned out to be the case. This law was much assailed, and at the same time much misunderstood. The objection that vapour did not rise so rapidly in air as in a vacuum seemed to him a strong one, which he did not quite get over, but considered it as presenting the same difficulty to all theories of the solution of water in air.

The law was stated too broadly, it did not even allow room for the impenetrability of matter to have its due place, and many persons supposed it to mean that a space filled with one gas, might be filled with an equal quantity of another.

He subsequently stated these two propositions in the following form, which he published in the second edition of his "New system of chemistry," when, after many years, he reviewed himself and his reviewers. p. 191, Part I., 1842.

"1. The diffusion of gases through each other is effected by means of the repulsion belonging to the homogeneous particles; or to that principle which is always energetic to produce the dilatation of the gas.

"2. When any two or more mixed gases acquire an equilibrium, the elastic energy of each against the surface of the vessel or of any liquid, is precisely the same as if it were the only gas present occupying the whole space, and all the rest were withdrawn."

There is no doubt that the law had been hastily expressed: explaining some points, it contradicted others. The phenomenon of the mixing of gases is easily explained, if we admit the constant intestine motion of the particles to be a necessary condition of the existence of a body in a gaseous state. (See a paper "On the changes of temperature produced by the rarefaction and condensation of air," by J. P. Joule. Phil. Magaz., May, 1845.)

The second essay is on the force of steam or vapour. He

gives a long table of the force of aqueous vapour at different temperatures, from 40° to 325°. Between 32° and 312° the numbers are given from experiment; above and below these limits the numbers are from calculation. These tables were afterwards modified by himself, and others have also reduced them to greater accuracy. He objects to the tables from water and alcohol given by M. Betancourt in 1790, and to that in the Encyclopædia Britannica, because the authors had assumed the force of that from water, at 32°, to be nothing. This constituted one of the steps which the subject made in its rather retarded progress.

He gives a series of experiments on the power of vapour from liquids, supporting his conclusions by experiments on ether, alcohol, water of ammonia, solution of muriate of lime, mercury, and sulphuric acid, and says "That the variation of the force of vapour from all liquids is the same for the same variation of temperature, reckoning from vapour of any given force; thus assuming a force equal to 30 inches of mercury as the standard, it being the force of vapour from any liquid boiling in the open air, we find aqueous vapour loses half its force by a diminution of 30° of temperature; so does the vapour of any other liquid lose half its force by diminishing its temperature 30° below that in which it boils, and the like for any other increment or decrement of heat." p. 564.

When speaking of vapour of water in air, he says "the results of all agree in one general rule or principle, which is this; let 1 represent the space occupied by any kind of air of a given temperature, and free from moisture; $p =$ the given pressure upon it in inches of mercury; $= f =$ the force of vapour from any liquid in that temperature in vacuo; then the liquid being admitted to the air, an expansion ensues, and the space occupied by the air becomes immediately and in a short time $= 1 + \frac{f}{p-f}$; or which is the same thing $= \frac{p}{p-f}$. Thus in water for instance, let $p = 30$ inches $f = 15$ inches

to the given temperature 180°. Then $\frac{p}{p-f} = \frac{30}{30-15} = 2$ for the space; or the air becomes of twice the bulk." p. 572. "In short, in all cases the vapour arises to a certain force according to temperature, and the air adjusts the equilibrium by expanding and contracting as may be required."

"The notion of a chemical affinity subsisting between the gases and vapours of different kinds cannot at all be reconciled to these phenomena." p. 574.

This notion of chemical affinity holding the gases in solution had begun to die out.

In essay third, "*On evaporation*," he concludes that the quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same. He adds a "table shewing the force of vapour, and the full evaporating force of every degree of temperature from 20° to 85°, expressed in grains of water that would be raised per minute from a vessel of six inches in diameter, supposing there were no vapour already in the atmosphere." p. 585. He obtained the evaporation from a surface when the air was still and when in motion. He adds also rules to find the amount of water that can be evaporated from a given surface when the temperature of the air is given, and the condensing point, and to find the force of the aqueous vapour.

The fourth essay on the expansion of elastic fluids by heat proves the law already stated.

The position of the question when he took up the subject may best be explained by himself, he says, p. 595, "The principal occasion of this essay is another on the same subject by Messrs. de Morveau and du Vernois, in the first vol. of the Annales de Chimie. It appearing to them that the results of the experiments of De Luc, Col. Roy, de Saussure, Priestley, Vandermonde, Berthollet, and Monge, did not sufficiently accord with each other; and that it would be of

importance to determine not only the whole expansion of each gas from two distant points, such as the freezing and boiling, but likewise whether that expansion be uniform in every part of the scale, they instituted a set of experiments expressly for those purposes. The result of which was; that betwixt the temperatures of 32° and 212°, the whole expansion of one gas differs much from that of another, it being in one instance about 4-10ths of the original, and in others, more than twelve times that expansion; and that the expansion is much more for a given number of degrees in the higher than in the lower part of the scale. These conclusions were so extremely discordant with and even contradictory to those of others, that I could not but suspect some great fallacy in them, and found it in reality to be the fact; I have no doubt it arose from the want of due care to keep the apparatus and materials free from moisture."

After giving his experiments on air, hydrogen, oxygen, carbonic and nitrous gas, in which "the small differences never exceeded six or eight parts, on the whole 345," he adds, "Upon the whole, therefore, I see no sufficient reason why we may not conclude that all elastic fluids under the same pressure expand equally by heat, and that for any given expansion of mercury, the corresponding expansion of air is proportionally something less, the higher the temperature."

"This remarkable fact that all elastic fluids expand the same quantity in the same circumstances, plainly shews that the expansion depends solely upon heat; whereas the expansion in solid and liquid bodies seems to depend on an adjustment of the two opposite forces of heat and chemical affinity, the one a constant force in the same temperature, the other a variable one, according to the nature of the body; hence the unequal expansion of such bodies. It seems, therefore, that general laws respecting the absolute quantity and the nature of heat, are more likely to be derived from elastic fluids than

from other substances." There is an admirable clear-sightedness in his short and rapid conclusions. The same law of equal expansion of gases was published six months later by Gay Lussac, and is often called by his name. Dr. Ure says the experiments were made by Gay Lussac with much more care and exactness, but the newest results obtained by Regnault by no means speak so in favour of Gay Lussac. The difference between his results and Dalton's were only trifling. Gay Lussac gave the expansion per degree at 480, Dalton 483, Regnault 491. In this country we have generally used Gay Lussac's for no sufficient reason. On the Continent Dalton has almost been entirely deprived of his merit, and is not even mentioned in connection with it in many French and German works: but such circumstances are unfortunately of constant occurrence. It is difficult to find the reason of this, but it happens so often that our countrymen are quite omitted in their works, that it must in a great measure arise from their neglect of our literature. This certainly must be the cause, as we find that both French and Germans of high name can treat latent heat without even mentioning the name of Black, whose claims are not even disputed; this last occurs even with the very systematic Gmelin. We can readily imagine how some of the other papers of Dalton have been overlooked as merely additions to a subject, whereas he who gives the polish and establishes the law has been allowed the entire credit. They were certainly put within the reach of inquirists, as he says in a letter quoted in Dr. Henry's life of him, p. 59. "My lately published essays on gases, &c., together with the more recent ones read at our society, and of which I gave the result in my late lectures, have drawn the attention of most of the philosophers of Europe. They are busy with them at London, Edinburgh, Paris, and in various parts of Germany, some maintaining one side and some another. The truth will surely out at last." Although not alluding specially

to the last mentioned memoir, this letter alludes to his investigations generally, which had been everywhere discussed.

On November 12th, 1802, he read to the Literary and Philosophical Society an "Experimental inquiry into the proportion of the several gases or elastic fluids constituting the atmosphere."*

These he made by weight, p. 257.

Azotic gas	75.55
Oxygenous gas	23.32
Aqueous vapour	1.03
Carbonic acid gas	10
	100.00

In another place we find, by bulk...... 79 azote.
21 oxygen.

In describing his Eudiometric process he has a few observations of great importance, indications of the direction in which he was moving, but given in such a way as to lead us to the conclusion that he had not yet seen their value; teaching us also that an idea of definite proportions may exist without any distinct nature of the completeness of the law of equivalents as it stands. At page 249 he says,—

"2. If 100 measures of common air be put to 36 of pure nitrous gas in a tube 3-10ths of an inch wide and 5 inches long, after a few minutes the whole will be reduced to 79 or 80 measures, and exhibit no signs of either oxygenous or nitrous gas.

"3. If 100 measures of common air be admitted to 72 of nitrous gas in a wide vessel over water, such as to form a thin stratum of air, and an immediate momentary agitation be used, there will, as before, be found 79 or 80 measures of pure azotic gas for a residuum.

"4. If in the last experiment, less than 72 measures of nitrous gas be used, there will be a residuum containing oxy-

* 1st vol. of Memoirs, new series, p. 244.

genous gas; if more, then some residuary nitrous gas will be found.

"These facts clearly point out the theory of the process: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity. In the former case *nitric* acid is the result, in the latter *nitrous* acid; but as both these may be formed at the same time, one part of the oxygen going to one of nitrous gas and another to two, the quantity of nitrous gas absorbed should be variable; from 36 to 72 per cent. for common air. This is the principal cause of that diversity which has so much appeared in the results of chemists on this subject."

In the paper on the expansion of elastic fluids, he had already, in a plate, shown that he was accustomed to view gases as composed of definite particles, having drawn each with a different form.

Immediately after this, January 28th, 1803, he read, an inquiry "On the tendency of elastic fluids to diffusion through each other."* This subject was first begun by Priestley. The memoir which he has written on the transmission of gases through porous vessels, entitled "Experiments relating to the seeming conversion of water into air," is certainly one of the most beautiful specimens of investigation that can anywhere be found. He there establishes the fact, that through porous vessels, gases pass one way, vapour of water and other liquids another; and observed, that the mercury in one experiment had risen 3½ inches above the level on the outside. He afterwards found that what could take place with "air and water, will be done with any two kinds of airs."

He failed, however, to make the next step, having said that it is probable "that if two kinds of air of very different specific gravities, were put into the same vessel with very great care, they might continue separate," although his

* Memoirs, Vol. I., New Series, p. 259.

own experiments justified a different opinion. Dalton too the subject up at this stage, and says the result "establishes this remarkable fact, that a lighter elastic fluid cannot rest upon a heavier, as is the case with liquids; but they are constantly active in diffusing themselves through each other until an equal equilibrium is effected; and that without any regard to their specific gravity, except so far as it accelerates or retards the effect according to circumstances."*

"The only apparatus found necessary, was a few phials and tubes with perforated corks; the tube mostly used was one 10 inches long, and of 1-20th inch bore; in some cases a tube of 30 inches in length, and 1-3rd inch bore was used; the phials held the gases that were subjects of experiment, and the tube formed the connection." p. 261. This tube was often a piece of tobacco pipe.

He believes that this proves his theory of elastic fluids to be correct, that gases are as a vacuum to each other, and it no doubt does favour it, especially as he added that they might be obstructed as a stream of water by a stony bed. Still this very explanation takes away much of the original meaning, and any of his difficulties as to the mutual action of gases must be cleared by further experiments, as has been the case with the laws of diffusion which have already been shewn to us by Professor Graham. There is no doubt that Dalton's expression is an useful attempt to grasp a great difficulty, not yet grasped, we shall see him returning to it again in the next paper.

On October 21st, 1803, he read to the Literary and Philosophical Society, another investigation "On the absorption of gases by water and other liquids." p. 271., Vol. I., New Series.

In this he says, 2. "If a quantity of water freed from air be agitated in any kind of gas not chemically uniting with water, it will absorb its bulk of the gas, or otherwise a

* Page 260, Vol. I., New Series.

part of it, equal to some one of the following fractions, namely, 1-8th, 1-27th, 1-64th, 1-125th, these being the cubes of the reciprocals of the natural numbers 1, 2, 3, &c.;" This has not found general assent, nor can it flow from any known natural law; indeed if it were true it would not shew itself by the usual mode of experimenting, as we can readily imagine one part of the water having 1-4th, another 1-5th, both being distinct parts of the whole, but so mixed with each other in the water that no result is perceived.

4. "If a quantity of water free from air be agitated with a mixture of two or more gases, such as atmospheric air, the water will absorb portions of each gas the same as if they were presented to it separately in their proper density."

5. "If water impregnated with any one gas (as hydrogenous) be agitated with another gas equally absorbable (as azotic), there will apparently be no absorption of the latter gas; just as much gas being found after agitation as was introduced to the water; but upon examination the residuary gas will be found a mixture of the two, and the parts of each, in the water, will be exactly proportional to those out of the water."

"10. Pure distilled water, rain and spring water, contain nearly their due share of atmospheric air; if not, they quickly acquire that share by agitation in it, and lose any other gas they may be impregnated with. It is remarkable however that water by stagnation in certain circumstances loses part or all of its oxygen, notwithstanding its constant exposition to the atmosphere. This I have uniformly found to be the case in my large wooden pneumatic trough, containing about 8 gallons. * * * * The quantity of azotic gas is not materially diminished by stagnation, if at all." He has not here considered the action of the organic substances.

Theory of the absorption of gases by water. p. 283.

"1. All gases that enter into water and other liquids, by means of pressure, and are wholly disengaged again by the

removal of that pressure, are mechanically mixed with the liquid, and not chemically combined with it."

He had already mentioned Dr. Henry's discovery, that the quantity of gas absorbed is as the density or pressure.

"2. Gases so mixed with water, &c., retain their elasticity or repulsive power amongst their own particles, just the same in the water as out of it, the intervening water having no other influence in this respect than a mere vacuum."

"3. Each gas is retained in water by the pressure of gas of its own kind incumbent on its surface abstractedly considered, no other gas with which it may be mixed having any permanent influence in this respect."

"4. When water has absorbed its bulk of carbonic acid gas, &c., the gas does not press on the water at all, but presses on the containing vessel just as if no water were in. When water has absorbed its proper quantity of oxygenous gas, &c., that is, 1-27th of its bulk, the exterior gas presses on the surface of the water with 26-27ths of its force, and on the internal gas with 1-27th of its force, which force presses upon the containing vessel, and not on the water. With azotic and hydrogenous gas the proportions are 63-64ths and 1-64th respectively. When water contains no gas, its surface must support the whole pressure of any gas admitted to it, till the gas has in part forced its way into the water."

"5. A particle of gas pressing on the surface of water is analogous to a single shot pressing upon the summit of a square pile of them. As the shot distributes its pressure equally amongst all the individuals forming the lowest stratum of the pile, so the particle of gas distributes its pressure equally amongst every successive horizontal stratum of particles of water downwards, till it reaches the sphere of influence of another particle of gas. For instance, let any gas press with a given force on the surface of water, and let the distance of the particles of gas from each other be to those of water as 10 to 1, then each particle of

gas must divide its force equally amongst 100 particles of water, as follows: It exerts its immediate force upon 4 particles of water; those 4 press upon 9, the 9 upon 16, and so on according to the order of square numbers, till 100 particles of water have the force distributed amongst them; and in the same stratum each square of 100, having its incumbent particle of gas, the water below this stratum is uniformly pressed by the gas, and consequently has not its equilibrium disturbed by that pressure."

"When water has absorbed 1-27th of its bulk of any gas, the stratum of gas on the surface of the water presses with 26-27ths of its force on the water, and with 1-27th of its force on the uppermost stratum of gas in the water; the distance of the two strata of gas must be nearly 27 times the distance of the particles in the incumbent atmosphere, and 9 times the distance of the particles in the water. This comparatively great distance of the inner and outer atmosphere arises from the great repulsive power of the latter, on account of its superior density, or its presenting 9 particles of surface to the other 1. When 1-64th is absorbed, the distance of the atmospheres becomes 64 times the distance of two particles in the outer, or 16 times that of the inner. The annexed views of perpendicular and horizontal strata of gas in and out of water will sufficiently illustrate these positions."*

"7. An equilibrium between the outer and inner atmospheres can be established in no other circumstance than that of the distance of the particles of one atmosphere being the same or some multiple of that of the other; and it is probable the multiple cannot be more than 4. For in this case the distance of the inner and outer atmospheres is such as to make the perpendicular force of each particle of the former or those particles of the latter that are immediately subject to its influ-

* A plate accompanied this.

ence, physically speaking, equal; and the same may be observed of the small lateral force."

"8. The greatest difficulty attending the mechanical hypothesis arises from different gases observing different laws. Why does water not admit its bulk of every kind of gas alike? This question I have duly considered, and although I am not yet able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases; those whose particles are lightest and single being least absorbable, and the others more, according as they increase in weight and complexity — (subsequent inquiry made him think this less probable). An inquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new; I have lately been prosecuting this inquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments."

He then gives a list of *relative weights* of 21 substances, constituting the first attempt to form a table of atomic weights.

"Table of the relative weights of the ultimate particles of gaseous and other bodies.

Hydrogen	1
Azot	4.2
Carbone	4.3
Ammonia	5.2
Oxygen	5.5
Water	6.5
Phosphorus	7.2
Phosphuretted hydrogen	8.2
Nitrous gas	9.3
Ether	9.6
Gaseous oxide of carbone	9.8

Nitrous oxide	13.7
Sulphur	14.4
Nitric acid	15.2
Sulphuretted hydrogen	15.4
Carbonic acid	15.3
Alcohol	15.1
Sulphureous acid	19.9
Sulphuric acid	25.4
Carburetted hydrogen from stagnant water	6.3
Olefiant gas	5.3"

I have given as much as possible, in his own words, the most important points attended to by Dalton up to this date. It was not my intention to inquire into the particulars relating to the novelty of the views taken by him, except on the atomic theory, and have therefore purposely left out any such opinions as might require discussion; nor have I shewn in all cases where advancing science has differed from his results. Some things in the papers alluded to were bold and strikingly new, some things are improvements on the old, some are mere re-statements of the old, but all is done in a firm, clear, and determined manner, as by a master in the business, going to the real point of difficulty in every case, and at all times avoiding unimportant details or vain ornament. He drives on like a new settler, and clears the ground before him, leaving it rather rugged it is true; neverthelesss it is resolutely cleared.

CHAPTER III.

DALTON'S SOCIAL LIFE.

We now approach the most important discovery of Dalton, and before entering upon it, it may be well to acquaint the reader with the general character and appearance of the man in his vigour. It is not my intention, as before stated, to amuse myself or readers with many little incidents of his life, nor can we gain by looking at such a character apart from the student of nature; but it is needful to give some slight account of the appearance and habits of the agent by which such valuable knowledge of natural law has been gained. On his habits, Miss Johns's Journal, lent me freely by Mr. Woolley, is the best authority. The Rev. W. Johns, once a colleague of Dalton's at the academy, had a school in George-street, near the Literary and Philosophical Society, which had given up a portion of its room to Dalton. In the autumn of 1804, Mrs. Johns saw him casually pass, and asked him why he never came to see them. Dalton said, "I do not know; but I will come and live with you, if you will let me." He did so, and took possession of the only bedroom at liberty, sitting with the family. In this family he lived for twenty-six years in the greatest amity, until Mr. Johns, giving up the school, sought a purer air in the suburbs of the town.

The portrait which is appended to this memoir, is from a picture by Allen, presented to the society by that painter on the occasion of Dalton being made president. It represents him in the vigour of life, and must of course be a more suitable representation of the man than those taken in old age, although one at least of those by Stephenson is an excellent portrait of a late period, and a beautiful engraving.

He was rather above the middle size, five feet seven inches. Mr. Giles, who read a memoir of him to the Manchester Society, and who was his pupil, says that "he was robust, athletic, muscular, and stooped slightly as if hasting forward, for he was a rapid walker. His countenance was open and manly. His voice was deep and gruff; and his lectures were by no means interesting, except to those who were satisfied with matter independent of style: he even spoke in a careless and mumbling manner." He was a Quaker, as has been said, and dressed in their peculiar manner, taking care that every article of dress should be of the finest texture, but avoiding the extreme of formality. In his general conversation he did not adopt their style; and never gave any opinion on religious subjects. His most intimate friends found him reserved on such points, although at times they found that there was in him great reverence and deep feeling. But he evidently did not think much on religious subjects, and seems not even to have formed strong opinions upon them, giving way to the opinions of those around him, like one unable or unwilling to form them for himself. If this were the case only with such subjects as are peculiarly religious, we might suppose that it arose simply from a want of agreement with current opinions which he was unwilling to disturb, but as he stood in the same attitude towards metaphysical opinions, we may fairly conclude that those faculties which discuss the moral and intellectual history and position of man, were not highly developed in Dalton. It would not be wise to conclude that they were weak, because we find that he had a great power of concentration, and in his ardent study of a subject he seems to have allowed the rest of his mind to be satisfied with meditative culture. In early life he seemed inclined to answer moral and metaphysical questions in the *Gentleman's Magazine;* and occasionally a lady would induce him to write a few lines of such poetry as a well-educated man is generally able to write. But his life was

spent in his laboratory, and all subjects not connected with his pursuits were much neglected; he might have said of their cultivation, as he said when asked why he never got married, "I never had time."

He rose at about eight o'clock in the morning; if in winter, went with his lantern in his hand to his laboratory, lighted the fire, and came over to breakfast, when the family had nearly done. Went to the laboratory and staid till dinner time, coming in a hurry when it was nearly over, eating moderately and drinking water only. Went out again and returned at about five o'clock to tea, still in a hurry, when the rest were finishing. Again to his laboratory till nine o'clock when he returned to supper, after which he and Mr. Johns smoked a pipe, and the whole family seems much to have enjoyed this time of conversation and recreation after the busy day. Dr. Schunck, who dined there as a child at school, says he never appeared in a hurry.

He was rather silent, especially if the company were large, but an attentive listener, whilst he occasionally introduced some short sentence of dry humour. With a few of his intimate friends he enjoyed much a lively conversation, but does not seem to have been fitted for dealing with men assembled in large numbers, either in public or private. This did not arise from any want of self-possession, which is said never to have been known to be ruffled: an illustration of this is given in the following. When in using the air-pump at a lecture a glass vessel burst, making a considerable noise and causing the ladies to scream, he simply said, "that is more than I intended, it's broken," and went on again. His disinclination to speak made him, as a teacher, by no means communicative; he allowed his pupils to learn, and willingly answered a question, but during the most of the time he was attending to his experiments, thinking, probably, that they were much better off than he ever was to have some one to apply to if a difficulty arose.

His habits were careful and economical, some say parsimonious, but he was by no means wanting in generosity, and gave fifty pounds to the building of the new Meeting House, at a time when he certainly could have had but very little. Such men do not often seek amusements, and he had only one, a remnant of Cumberland, which he seemed never to forget, at the same time also a wholesome exercise. Every Thursday afternoon, about two o'clock, he went outside the town to the "Dog and Partridge," now far within Manchester, and played a few games at bowls. This he seems to have thoroughly enjoyed, watching the bowls with the greatest anxiety, and by his constant movements indicating, as people are apt to do, the way in which they wish the bowl to move, as if endeavouring to influence it. He shewed even there a glimmer of the latent enthusiasm of his mind. He played a fixed number of games and then ceased, took tea at the inn, smoked his pipe, and went to his laboratory. Between twelve and one he usually went to the Portico to read the newspapers, but did not strongly speak on political subjects, so that even the Johns family did not for a long time know that he was Conservative in politics. At the same time Mr. Giles says he was a Liberal, always voting for Liberals, so that we may call him a liberal Conservative, which however indefinite, is somewhat the character of such a man, considerate, desirous of improvement, but not inclined to violent change.

His great delight was to visit the hills of Cumberland where he first studied the clouds and the aurora, and when the usual day of June had come, old Matthew Jobson came out of his cottage under the slopes of Helvellyn, and looked out for Dalton and his instruments. He ascended the hill from thirty to forty times during his life, walking rapidly and with ease, generally keeping before any party who accompanied him, so as on one occasion to have brought out the exclamation from a friend, "John, I wonder what thy legs are made of." In later

life occasionally he was accompanied by some of the younger members of the family to whom he was now as a relation, and we can well believe Miss Johns, when she says, "that to those who have seen him only on ordinary occasions, it is impossible to convey an idea of his enthusiasm on those occasions. He never wearied." Jonathan Otley, the veteran guide, at Keswick, who has spent his life mapping, describing, and showing the country around, often accompanied Dalton, and he has in a journal given an account of some of their excursions. These were undertaken partly with a view of "bringing into exercise a set of muscles, which would otherwise have grown stiff," as Dalton said, and partly to make meteorological observations, or to bring down air for analysis from the highest points of the county, gas from the floating island, and minerals from every hill.

In order to give his habits of thought on objects not scientific, a few of his letters may be introduced with advantage; at the same time they will give an account of some portions of his life, which would lose much of their interest if the words of another were alone used.

Having been invited to lecture at the Royal Institution, he thus describes his visit in a letter to his brother, February 1st, 1804. His first visit to London had been made in 1792.

"Dear Brother, I have the satisfaction to inform thee that I returned safe from my London journey, last seventh day, having been absent six weeks. It has, on many accounts, been an interesting vacation to me, though a laborious one. I went in a great measure unprepared, not knowing the nature and manner of the lectures in the institution, nor the apparatus. My first was on Thursday, December 22nd (1803), which was introductory, being entirely written, giving an account of what was intended to be done, and natural philosophy in general. All lectures were to be one hour each, or as near as might be. The number attending were from one to three hundred of both sexes, usually more than half men. I was

agreeably disappointed to find so learned and attentive an audience, though many of them of rank. It required great labour on my part to get acquainted with the apparatus and to draw up the order of experiments and repeat them in the intervals between the lectures, though I had one pretty expert to assist me. We had the good fortune, however, never to fail in any experiment, though I was once so ill prepared as to beg the indulgence of the audience, as to part of the lecture, which they most handsomely and immediately granted me by a general plaudit. The scientific part of the audience was wonderfully taken with some of my original notices relative to heat, the gases, &c., some of which had not before been published. Had my hearers been generally of the description I had apprehended, the most interesting lectures I had to give, would have been the least relished, but as it happened, the expectations formed had drawn several gentlemen of first-rate talents together; and my eighteenth, on heat, and the cause of expansion, &c., was received with the greatest applause, with very few experiments. The one that followed was on *mixed elastic fluids*, in which I had an opportunity of developing my ideas, that have already been published on the subject more fully. The doctrine has, as I apprehended it would, excited the attention of philosophers throughout Europe. Two journals in the German language, came into the Royal Institution, whilst I was there, from Saxony, both of which were about half filled with translations of the papers I have written on the subject, and comments on them. Dr. Ainslie was occasionally one of my audience, and his sons constantly: he came up at the concluding lecture, expressed his high satisfaction, and he believed it was the same sentiment with all or most of the audience. I was at the Royal Society one evening, and at Sir Joseph Banks's another. This gentleman I had not, however, the pleasure of seeing, he being indisposed all the time I was in London.

"I saw my successor, William Allen, fairly launched; he

gave his first lecture on Tuesday preceding my conclusion. I was an auditor in this case, the first time, and had an opportunity of surveying the audience. Amongst others of distinction the Bishop of Durham was present.

"In lecturing on optics I got six ribbands, blue, pink, lilac, and red, green, and brown, which matched very well and told the curious audience so. I do not know whether they generally believed me to be serious, but one gentleman came up immediately after and told me he perfectly agreed with me: he had not remarked the difference by candle light."

This letter concludes characteristically by "The rain has been 27½ inches last year."

Like many students whose nerves are not easily affected, Dalton liked tobacco. A thorough explanation of its action on various constitutions seems to have hitherto escaped the research of medical men, most of them being content either to admire it, or to detest it, according as it may suit themselves. The following letter to Mr. John Rothwell gives Dalton's taste.

London, Jan. 10th, 1804.

"I was introduced to Mr. Davy, who has rooms adjoining mine in the Royal Institution: he is a very agreeable and intelligent young man, and we have interesting conversations in an evening: the principal failing in his character is, that he does not smoke. Mr. Davy advised me to labour my first lecture: he told me the people here would be inclined to form their opinion from it; accordingly I resolved to write my first lecture wholly; to do nothing but to tell them what I would do, and enlarge on the importance and utility of science. I studied and wrote for nearly two days, then calculated to a minute how long it would take me reading, endeavouring to make my discourse about fifty minutes. The evening before the lecture, Davy and I went into the theatre: he made me read the whole of it, and he went into the furthest corner; then he read it, and I was the audience; we criticised upon

each other's method. Next day I read it to an audience of about 150 or 200 people, which was more than were expected. They gave a very general plaudit at the conclusion, and several came up to compliment me on the excellence of the introductory. Since that time I have scarcely written anything; all has been experiment and verbal explanation. In general my experiments have uniformly succeeded, and I have never once faltered in the elucidation of them. In fact, I can now enter the lecture room with as little emotion nearly as I can smoke a pipe with you on Sunday or Wednesday evenings."

In 1807 he gave a similar course of lectures in Edinburgh, on which the following addressed to his friend, the Rev. W. Jones, may be read with interest.

Edinburgh, April 19th, 1807.

"Respected Friend,

As the time I proposed to be absent is nearly expired, and as my views have recently been somewhat extended, I think it expedient to write you for the information of enquirers. Soon after my arrival here I announced my intention by advertisement of handbills; I obtained introduction to most of the professional gentlemen in connection with the college, and to others not in that connection, by all of whom I have been treated with the utmost civility and attention; a class of eighty appeared for me in a few days; my five lectures occupied me nearly two weeks; they were finished last Thursday, and I was preparing to leave the place, and return by Glasgow, to spend a week. But several of the gentlemen who had attended the course represented to me that many had been disappointed in not having been informed in time of my intention to deliver a course, and that a number of those who had attended the first course would be disposed to attend a second. I have been induced to advertise for a second, which, if it succeeds, will commence on Wednesday, the 22nd, and be continued daily, till the conclusion. This will detain me a

week yet; I then set off for Glasgow, where I may be detained for a week or more, so that I see no probability of reaching Manchester before the beginning of May, to which I look forward with some anxiety. Hitherto I have been most highly gratified with my journey; it is worth coming 100 miles merely to see Edinburgh. It is the most romantic place and situation I ever saw; the houses touch the clouds; at this moment I am as high above the ground as the cross on St. James's spire; yet there is a family or two above me; in this place they do not build houses side by side as with you, they build them one upon another, nay, they do what is more wonderful still, they build one street upon another; so that we may in many places see a street with the people in it, directly under one's feet, at the same time that one's own street seems perfectly level and to coincide with the surface of the earth. My own lodgings are up four flights of stairs from the front street, and five from the back. I have just 100 steps to descend before I reach the real earth. I have a most extensive view of the sea; at this moment I see two ships; and mountains across the Firth of Forth, at the distance of thirty miles; to look down from my windows into the street at first made me shudder, but I am now got so familiar with the view, that I can throw up the window and rest on the wall, taking care to keep one foot as far back in the room as I can, to guard the centre of gravity. The walks about Edinburgh are most delightfully romantic. The weather is cold; ice every morning, and we had a thick snow a few days ago. Upon walking up on to an eminence I observed all the distant hills white; the nearer ones speckled; I saw five or six vessels just touching the horizon; they seemed to be about ten or twelve miles off, and their white sails looked like specks of snow on the sea. I saw a dozen or two at anchor in the river, and a most charming view of the Fifeshire hills on the other side of the Firth. Adieu. My best regards to you all."

J. Dalton.

Again, from London, December 27th, 1809, when giving another course of lectures, he writes to Mr. Jones, after some pleasant gossip about his fellow-travellers:—

"On Tuesday I spent greater part of the day (morning they call it here) with Mr. Davy in the laboratory of the Royal Institution. Sir I. Sebright, M.P., who is becoming a student of chemistry, was present. We had a long discussion. In the evening I walked three miles into the city, to Pickford's, to look after my boxes; I found them there, but as they promised to send them next day I did not take them. They disappointed me. On Wednesday I attended Mr. Bond's lecture on astronomy, and prepared for mine the next day. On Thursday, at two, I gave my first lecture. Mr. Pearson, a former acquaintance, went home with me after the lecture, and we had a long discussion on mechanics. Mr. Davy had invited me to dine with the club of the Royal Society, at the Crown and Anchor, at five o'clock, but I was detained till near six; I got there and called Davy out; all was over; the cheese was come out. I went, therefore, to the nearest eating-house I could find to seek a dinner; looking in at a window I saw a great heap of pewter plates and some small oblong tables covered with cloths. I went in and asked for a beefsteak; "no." What can I have? "boiled beef." Bring some immediately. There was nothing eatable visible in the room, but in three minutes I had placed before me a large pewter plate covered completely with a slice of excellent boiled beef swimming in gravy, two or three potatoes, bread, mustard, and a pint of porter. Never got a better dinner. It cost me 11½d. I should have paid 7s. at the Crown and Anchor. I then went to the Royal Society and heard a summary of Davy's paper on chemistry, and one of Home's on the poison of the rattlesnake: Sir J. Banks in the chair. Davy is coming very fast into my views on chemical subjects. On Friday I was preparing for my second lecture. I received a visit from Dr. Roget. On the evening I was

attacked with sore throat. I sweated it well in the night with cloathing, but it was bad on Saturday, and I was obliged to beg a little indulgence of my auditors on the score of exertion. However, I got through better than I expected. I kept in on Sunday and Monday and got pretty well recruited. On Tuesday I had my third lecture, after which I went to dine at a tavern to meet the chemical club. There were five of us, two of whom were Wollaston and Davy, secretaries of the Royal Society; we had much discussion on chemicals. Wollaston is one of the cleverest men I have yet seen here. To-day, that is Thursday (for I have had this letter two or three days in hand), I had my fourth lecture. I find several ingenious and inquisitive people of the audience. I held a long conversation to-day with a lady on the subject of rain-gauges. Several have been wonderfully struck with Mr. Ewart's doctrine of mechanical force. I believe it will soon become a prevalent doctrine. I should tell Mrs. J. something of the fashions here, but it is so much out of my province, that I feel rather awkward. I see the belles of New Bond-street every day, but I am more taken up with their faces than their dresses. I think blue and red are the favourite colours. Some of the ladies seem to have their dresses as tight round them as a drum, others throw them round them like a blanket. I do not know how it happens, but I fancy pretty women look well any how.

I am very regular with my breakfast, but other meals are so uncertain that I never know when or what. Hitherto I have dined at from two to seven o'clock; as for tea I generally have a cup between nine and ten, and, of course, no supper. I am not very fond of this way of proceeding. They say things naturally find their level, but I do not think it is the case in London. I sent for a basin of soup the other day before I went to lecture, thinking I should have a good threepenny worth, but I found they charged me one shilling and ninepence for a pint, which was not better than some of our

Mary's broth. Of course, I could not digest much more of the soup."

Again, the year after,

London, Jan. 29th, 1810.

"You may perhaps have heard from Dr. Henry that I have been nearly as ill as formerly, that I have been nearly poisoned since I came here. I had been about three weeks when I discovered it was the porter which produced the effects.* I have not had a drop since, and have never had any more of the symptoms.

"I have had a pretty arduous work, as you may imagine, having had three lectures to prepare each week, to attend two others, and to visit and to receive visits occasionally besides. I find myself just now in the focus of the great and learned of the metropolis. On Saturday evening I had a discussion with Dr. Wollaston, and a party at Mr. Lowry's. On Sunday evening, last night, I was introduced to Sir Joseph Banks, at his house, by Sir John Sebright. Sir Joseph said, 'Oh, Mr. Dalton, I know him very well; glad to see you; hope you are well, &c.' There were forty or more of the leading scientific characters present, many of whom were my previous acquaintance, such as Sir Charles Blagden, Drs. Wollaston, Marcet, Berger, and Roget; Messrs. Cavendish, Davy, Tennant, Lawson, &c.; we had conversation for about an hour or more in Sir Joseph's library, when the company dispersed. To judge from the number of carriages at the door, it might be a court levee.

"I paid a visit, in company with Dr. Lowry, to Dr. Rees, the other day; we spent an hour in conversation in the doctor's library. The doctor seems a worthy philosopher of the old school; his evening lucubrations are duly scented with genuine Virginia."

* Lead had been found in it. This was probably owing to the use of lead pumps, a very common and dangerous custom, whether used as is commonly the case in public-houses, at least in this neighbourhood, to pump the malt liquor from the cellar, or for water for domestic supply.

Sir Humphrey Davy's opinion of Dalton, given in Dr. Henry's recent life, seems to contain rather harsh and unpleasant expressions, and I think scarcely fair suggestions. He could not have known the man otherwise than externally; nor does he seem to have known well the history of his discoveries. I shall allude to it more when speaking of Dalton as a philosopher; as to the man I only collect the opinions of others, and give a few examples of his character from his letters and actions. His brother, Dr. Davy, also says, "Mr. Dalton's aspect and manner were repulsive. There was no gracefulness belonging to him. His voice was harsh and brawling; his gait stiff and awkward; his style of writing and conversation dry and almost crabbed. In person he was tall, bony, and slender. (1809-10.) He never could learn to swim; on investigating this circumstance he found that his specific gravity was greater than that of water, and he mentioned this in his lectures on natural philosophy in illustration of the capability of different persons for attaining the art of swimming." But he adds, "independence and simplicity of manner and originality were his best qualities. Though in comparatively humble circumstances he maintained the dignity of the philosophical character." So many "best" qualities are seldom found in one man.

This word *brawling* was unintelligible to me until Dr. Schunck suggested *drawling*, as the true meaning. Brawling is unintelligible in connection with such a retiring man. Dr. Schunck says, "no one who saw him could call his appearance repulsive. I recollect him from my childhood and never saw it; and children are very susceptible to repulsive appearance in people. He was, I think, good looking; only his deep set eyes were against him." At any rate, there is no connection between brawling and Dalton. When I saw him his mind was quite broken down; but I agree with my friend, Dr. Schunck, that he was agreeable to look upon.

minutes; after walking up a steep hill, we came to a row of most delightfully situated houses, on an eminence, looking down on the city and the adjacent country. Whether by night or by day it is delightful; at night, we see the lights of the city forming a circle around us; the general effect is most curious. A darkish cloud hung over the whole foreground; the lights from London illuminated that part over it, whilst on this side it was dark, and on the other side dark, so as to give the appearance of a luminous cloud, interposed between two black clouds. The night was still and fine, and Mr. L. promised me I should hear the nightingale; for one or two sung every night from a grove, 100 yards below his house. We smoked our segars till twelve, then looked out and listened; all silent, no nightingale. We went to bed about three, I was awoke by the melodious song of the nightingale, which continued, without any interruption, till four o'clock. Soon after three I also heard the second and third best singing birds that we have, according to Mr. Blackwall, and the first, or nightingale, all singing together. My bedroom window fronted the S.E. In the morning, at eight, I took hold of the blind string with one hand, and put my other to the opposite side to help it up, as usual, with most others; but to my surprise it spun up to the top of the window of its own accord, in a moment, and such a view as I never witnessed presented itself, except at St. Cloud, near Paris. For half a mile before me rose the tops of trees, from a beautiful grove, belonging to Mrs. Coutts, her house chimneys popping up on the right; over the tops of the trees the country presented itself, interspersed with houses, and beyond, London with its spires; St. Paul's dome, like Helvellyn, right in the front; the river, the Kent hills over London, &c., &c., the sun shining clear, the birds singing, &c., &c. Quite at the foot of my window, a kind of verandah, entwined with shrubs below, about 100 yards square of descending ground, so covered with shrubs as to hold two or three shady seats, and some

large shrubs, looking like a wilderness, and in the middle a little plot of green ground and a knot of flowers. In short, it was multum in parvo with a witness. This is the very house occupied by the late Mr. Joyce."

To Mr. Johns, when absent from Manchester.

July 2nd, 1825.

"Yesterday I dined at Dr. Henry's, meeting Professor Almroth, of Stockholm, and a party, together with his own young family and Miss Bailey. Professor Almroth breakfasted with me this morning. He is well read in Shakespere, Sir Walter Scott, and the German literature, as well as in *Shimistry*.

"You will have a new chapel to go to when you come here. The great water hole opposite has now a chapel on it, for Baptists, they say. * * * * I am nearly ready for a jaunt, but whether north or west I do not know till my stick falls."

We see from these letters that he was accustomed to lecture occasionally in all parts of the country, when invited, and we see also enough to let us judge a little of his temper. They are full of very simple kindly feeling, the very act of writing home so many details betokens a disposition to please, and entirely precludes every accusation of vanity or that absurd appearance of separation from his fellows, which we find so frequently the production of bigotry and of ignorance, but which we too often call by the name of dignity. We find him exceedingly pleased with the attentions of scientific men; he had, of course, frequent visits from such as came to Manchester, and foreigners of distinction seem to have pleased him as much as the young members of the family where he resided. At the same time he was tenacious of his opinions, and we find that in describing his conversations with men of science, he generally calls them *discussions*, and generally on scientific subjects we find him too much standing up for his own rights, as his results appeared to him to be. This

arose from a peculiarity of mind which seems to have been too much developed in him, as in those also where observation and experience are so tenaciously remembered as to become the only groundwork of opinion, and where the arguments of others are as mere fiction, having no influence upon the reasoning powers. Acting in this spirit, he discouraged reading, and prevented the Literary and Philosophical Society from obtaining a sufficient supply of books. He said, "I could carry all the books I have ever read on my back."* In this, he was evidently forgetting how diverse were the faculties of mankind, and acting also in ignorance of the fact, that he was himself suffering from a want of reading, although it is probably true that he was a gainer in another direction from the same cause. But in summing up features of characters, we find great antagonism in our results, so that we are in error when we make up a very uniform design from what is too often a hastily prepared patchwork, and known to be so by the owners themselves, but which they are prevented from completing by circumstances, if not by time, which limits the progress of all. He, in fact, is often the great man who allows himself to act onesidedly, not for his own pleasure or profit, but because the struggle which he has to maintain, needs all energies to be concentrated on one point of attack.

In this way we may view Dalton. We must see him also as a man having limited assistance from the knowledge of others, ignorant of many of the elegant and easy methods of procuring knowledge and illustrating facts which have become the common inheritance of universities, and accustomed to the society of few only who had similar studies; and when meeting with his contemporaries from capital cities, forgetting that they were not like him, completely immersed in the study of nature, but were also expected to cultivate the soireé and the dinner table. Still sociality was not entirely checked

* Mr. Woolley is my authority.

in Dalton, although it was under subjection to routine. This was seen in the regular manner in which he went on Sunday to dine, at Mayfield, with Mr. Neild, even if the host were not at home, and in the regular manner in which he joined the family of Mr. Johns every evening, enjoying the society of the younger members who had grown up from childhood under his eye, and under the same roof, and in the great pleasure he had in taking them with him in his summer excursions. It was a mild enjoyment, allowing of little enthusiasm, this was reserved for the sterner aspects of nature on the summits of Scawfell or Helvellyn; but it was his nature to be calm, a violent life does not suit the inquirer into nature.

He was simple in his habits by nature and by education, but still more so from his pursuits. Such are always found to be disturbed when wealth, by enlarging the establishment, claims too much the care of the possessor, and when work, ceasing to be urgent, allows us to imagine that the cultivation of an acquaintance is the great business of our life. To the courtly especially, he seemed morose, but that it was merely a question of form, and not of inward feeling, we see additional proof in the letter where he makes a similar complaint of Sir H. Davy, as lacking that geniality which he jocularly represents under the symbol of tobacco.

To a certain extent he was separated from society by the constancy of his work. Many have been separated, and separated themselves for idleness, but for work, few; and whilst the world is overflowing with those who would willingly give themselves to the pleasure of social intercourse, an occasional exception for a higher purpose stands forth as a subject for our admiration, surely not for a censure.

Dalton never married, he had not time, he said. In early life his position prevented him, in middle life constant work, when he seems to have been struggling for an independency, not knowing that it would come to him with ease as soon as his failing strength demanded it. This desire to become in-

dependent of his work induced in him an undue amount of care in the accumulation of his savings: we may consider them as the representatives of a certain amount of time entirely lost to all but his heirs. At the same it is to be remembered that he was accumulating his savings for a time, when he could no longer be able to work, and the simplicity and self-denial of such a course, instead of being worthy of blame, is a virtue, which, unfortunately, is not of sufficient occurrence. This virtue gave him the opportunity of showing kindness to many of his friends, and of helping such as were in need.

He was on terms of friendship with several ladies whom he greatly admired, and there is little doubt that in one case in early life the admiration was that of love. Whether the fact of the lady's engagement to another affected him for any length of time with disappointment, is what his reserved nature never told to any one, but we are left to guess that the attachment was strong, when late in life he could not read without emotion, and even tears, some verses the lady had written, or allow any one else to read them in the letter. Tears and emotion were rare with him, and leave us room enough for speculating on what might have been the inner romance of that life which externally seemed so simple and so contented with matters of fact.

His attention to ladies, and his great respect for their mental attainments, makes us still more inclined to refer to awkwardness of manner, the appearance of "repulsiveness and harshness," words that seem out of place when used in speaking of one so little disposed to offend.

The answer to his friend Mr. Gough, who attacked him on the subject of the atmosphere, shows great forbearance and innate nobleness of feeling, under circumstances in which every thing that is most bitter and severe is generally admitted to pardon.

In a town like Manchester, where exertions to extend the

material portion of civilization take the form in the minds of most men of a struggle for wealth, in which the original object is forgotten, it is a proof of simplicity and singleness of character when we find that he was never once led away by the glare of the princely fortunes around him. He gave lessons for very small fees, from 1s. 6d. to 2s. 6d. a lesson. He made analyses, and was consulted by manufacturers, probably the earliest in the district of that class of scientific men called "professional chemists" who have risen as a necessity of the time, and by private establishments have made some compensation for the lack of public institutions and professorships, in some countries so abundant, and have chiefly in their hands the connexion of the chemical arts with the science as it progresses.

Dalton's character as a man is then easy to understand; he was a simple inquirer into nature, his enthusiasm rose only in her presence, his life was devoted to her study. Abstracted in a great measure from the world in its social relations, he was sufficiently connected with it to have endeared himself to all those with whom he lived, and to have formed with some of his contemporaries the warmest friendships. The friends of his childhood were never forgotten, but more warmly remembered as he grew older; whilst he did not the less remember those that he learnt to know only since his manhood. Gentle at least in his spirit, his very solitary life and abstract mode of thinking had not allowed him time to modulate his voice to suit the ears of those accustomed to more polished society, and a certain rigidity of body, as well as of mind, caused the movements of both to have the appearance more of power than of grace. He was simple, temperate, and regular in his habits, never carried away by the feelings of the moment to indulge in luxuries to which he was unaccustomed, in all his actions avoiding excess, yielding to order and regularity as the only master passion which had power to carry him beyond what we may consider the just bounds of reason.

Although unwilling to offend, he was accustomed sometimes to give severe rebukes to ignorance when it pretended to know, and in this capacity alone do we find him ever creating a feeling of opposition in those who surrounded him, although even this was seldom, and only on great provocations, and chiefly in his character as President of the Philosophical Society, where the exercise of wise authority was expected and desired. Local memory tells us of several severe, but well deserved and not ill-natured rebukes.

In a life of labor, of experiments with weights and with numbers, it is seldom that the imagination flourishes, and so we find that literature was entirely neglected, and his own discoveries have not been illuminated by the radiance which it is in the power of some men to shed around the creations of their mind, but have been sent out dry and hard into the world to gather as they best might the life which he was certain of obtaining for them.

In this is the secret of many varying opinions about Dalton; this is the secret of his want of success in early life, of his remaining so long apart from scientific men, and of the disputes as to his originality. The "genial current of the soul" had been constitutionally stopped, and words were wanting to express his feeling, which at last seemed not even to struggle for utterance. He gave us knowledge, mere knowledge does not give life until it is become a familiar inmate of the mind, until we can see it in all or many of its aspects, until it becomes an object on which we delight to gaze, and seeing its beauty begin to surround it with results from the imagination. He that can do this, putting the results of science into a poetic form, and impressing them upon the mind, receives often more admiration than the originator, the exertions of whose whole life may probably be summed up in a sentence, and seen at a glance by the popular eye; but experience proves that the original work requires a persistance of effort and a clearness of conception, which are very rarely united.

True, Davy and Dumas add poetry and eloquence to sterling scientific vigour, whilst in Dalton we find only strength and rude simplicity; the glowing radiance which dazzles us in the writings, and even in the characters of some men, is wanting in him; but strength and simplicity are rare and valuable gifts, although we must look to their combination with beauty in its widest sense for the very highest standard of mankind.

CHAPTER IV.

HISTORY OF THE ATOMIC THEORY.

IDEAS OF MATTER UP TO THE TIME OF LUCRETIUS.

There is no chapter in the history of man more marvellous than that which deals with his conception of matter. There has been the greatest difficulty in all ages in comprehending its existence, and still more so in conceiving how it can be constituted of so many different substances. It seems, beyond expression, strange, that although himself made of matter, and exposed so frequently to the pain of living in regions covered with most inhospitable forms of it, or tossed about in an unmanageable ocean of the same, he should still for a long time confuse the conception of it with spiritual existence, and still longer fail to obtain any distinct idea of the cause of their diversity. I do not allude only to the metaphysical difficulties even now unsolved as to the existence of matter, but to those perhaps most apparent in the history of the physical sciences. The difficulties have begun with the savage who scarcely distinguishes the wood from his divinity, and still less divides substances into classes. They are difficulties which, in one form or other, have struggled long in man, and the progress of which may be seen perhaps most clearly in the Greek, but more or less in all nations who have thought on philosophical subjects, whilst the struggle in Europe in the middle ages was long and violent, occupying the most active minds of the age. It is strange to observe the pertinacity of man in deciding that matter is *one*, that all substances have the same substratum

without distinct facts as a proof, but relying on his reasoning powers alone; deciding that fire, air, earth, and water, are the same, although burnt out of his house by one, and drowned by the other, and to obtain a third, risking everything that he possesses. He has with difficulty been able to think of the facts before him, except under the influence of some previous conclusion, and pressed to the earth as he has been by physical difficulties, as well as by sensuality, he has continually clothed it with immateriality. Sometimes he appears as a spiritual being, from some higher state of his metempsychosis, with difficulty treating conceptions new to him about mere material things, or perhaps more like a material being oppressed with weakness of conception, grasping at more than he can understand, he has failed to see clearly the facts that of all others seem to stand most prominent before him.

The idea of matter representing the present stratum of knowledge obtained from experiment, and which chemists so long entirely missed, is, that there exist bodies which we cannot divide and call simple, that these by union among themselves form other bodies; that when united the original bodies are by no means lost, and may be again separated without losing any of their original indestructible properties. This statement is a simple relation of facts. These bodies may be farther divisible; they may be, and probably are, all convertible into one, but when we trace them further than our experiments warrant us, we go back to the position of the Greeks, who have already said nearly all that the mind seems able to attain to, unassisted by the study of nature. Indeed, this idea cannot be said to be in any way new, but one of the very oldest, although frequently lost, to be seen at intervals in fragments, but not until within the memory of man to obtain permanent ground in science: although inevitably doomed to prove only a portion of the truth, it is no less true in its own limits. So simple is the idea, requiring too such enormous labour and long time to

obtain, that it makes us readily believe that we may now be living under the grossest delusions which a little spark of genius might readily dispel, revealing to us a world entirely different to that which we are accustomed to see; and without a doubt this is to a great extent the case.

As an instance of the difficulty of arriving at a rational conception of material phenomena, but more especially in order to give a sketch of the history of the subject, I shall adduce the opinions shortly expressed of some of the most prominent thinkers of ancient times.

Much as has been written on this ancient part of philosophical history, a full as well as distinct account is still wanting. Most writers give us so much explanation, that we cannot understand them: the collected fragments would form a valuable volume. The physical has generally been given as a mere appendage to the metaphysical history.

Although the earliest opinions relate more directly to creation or cosmogony than to the nature of combinations, they still are interesting in connection with our subject, as they show us the early methods of viewing matter, and illustrate the difficulties as well as progress of the subject. Of the early Greek schools, where fragments only exist, I shall give a few particulars taken chiefly from Ritter and Tiedemann, not giving the original words of the ancient authors, some of which have been lost, and others greatly scattered.*

Thales considered the earth to be a living being, and the only primitive principle from which all things are formed to be water. He observed, no doubt, that where there is no water, there is no growth of vegetable or animal life. The vegetables are fed by the rain; they contain water in their juices, or they do not live. Animals dried up are dead. Life goes on in the world only with water, even the

* Ritter's "*Geschichte der Philosophie.*" I used the French edition. Tiedemann's "Geist der Spekulativen Philosophie."

land grows from it, the islands lift their heads out of water, and the continents repose on great waters.

The rising of the world from oceans was a theme in the still earlier cosmogonies of the east.

If all the solid things spring from water, and if water may pass into air as it seems to do in evaporating, then all things spring from water.

This idea is scarcely dead. Van Helmont believed he proved by experiment that plants grow from water, and without correct analyses, we should be obliged to decide in the same way.

Anaximenes believed the principle of all things to be found in the boundless air. We may reason thus with him, partly in his own words, and partly in ours. The world is limited, the land and water have a definite termination, the air alone is boundless. The air sometimes condenses from itself fierce winds, black clouds, rain, snow, and solid hail; these again are found on the earth as water, and are found to contain the solid world, and although this may rest in the water, the water itself rests in the infinite expanse of air. We breathe air, and live by doing so; when we cease, life ceases; the life and soul are air, which becomes in this way, not only the spirit which moves all things, but the source from which all things are produced, a living principle to the world as a whole, as well as to us.* "He believed in four principle degrees in the qualities of air, which responded to the common opinions of four elements; from these degrees, fire, air, water, and earth, were formed all the other properties of natural things."†

Diogenes, of Apollonia, believed also in "air being the origin of all things, but requires a greater variety of this element. For one is not the same as the other, for there are many varieties of air and many considerations; some is warmer, some colder, some drier, some moister, some calmer, some

* Ritter, Vol. I., p. 182. † Page 185.

more agitated, and it has many other alterations, caused both by its qualities and its substance; and the soul of all living things is air," &c. The air penetrates everywhere, it must be endued with intelligence, as all things are disposed with consummate wisdom. The differences in the sensible qualities of things are referred to condensation and expansion.

To prove that all things are one. "It appears that all that exists is merely the change of one and the same thing; and this is evident, that if all that is in the world, the earth and water, and other things, were different, and made essential changes, there could be no transformations among things."* This is correct reasoning, if we were convinced of transformations, and we must remember that without analysis, we must believe in such changes, as the plant seems to be transformed from water or earth. This reasoning marked a necessary step in the progress of chemistry.

We see here that properties have to be given to air in order to account for its diversities. These properties, among the most important, heat, are not clearly defined. The earth is precipitated from the air by condensation, but there is no explanation of condensation, so that after reasoning on these few principles, the simple atmospheric air which formed the commencement becomes something else, and a mystic air, a kind of substratum of air, is in reality what seems to be signified, as no common air could contain all the properties required. The same is wanted with water, which, after a while, has too much given it to do, to be only common water, and we find the very same thing occur with the rest of the four elements, as well as with the three alchemistic elements.

The soul of all things producing such admirable order, as Diogenes perceived, "it is astonishing that this doctrine did not conduct to the distinction of mind and matter, and that he conceived the principle of all things, even intellectual

* Page 188.

phenomena themselves, to be material." *—Ritter. But however this may be, it is not less astonishing than very much later opinions which distinctly gave material form to ideas. Diogenes was the last of this class who conceived the world to be a living thing, in a narrow sense, and each individual as for a time only able to live isolated, and to resist the influence of the external and greater life which in the end enveloped all.

We have seen air and matter made the first principles, and now we find Heraclitus, of Ephesus, fixing on fire, which before had been made to play an important part as heat, expanding or contracting all things. In the absence of any full exposition of his reasoning, let us rather attempt, as with the others, to complete it for ourselves. He seems to have thought that wherever there is warmth there is life, where heat comes there is motion and activity, it is the principle moving all things, a secret fire which gives life to all things, it produces air, as we see it constantly do when substances burn. Air again produces other elements. As fire produces air, so fire converts water into air, and heat causes those changes in the atmosphere which produce water, whilst by its action on the earth it produces land also, which is raised from the sea. As all things have preceded fire, so in the end must fire swallow up all things or all things will return to it. "The harmony of the world arises from contrary forces, as that of the lyre and the bow."† "The finest harmony is produced by opposites, and every thing is produced by strife." But as with the others, so with Heraclitus, fire was not common flame, but gradually became an expression of force, and we still use the word in this more spiritual sense.

In Anaximander, of Miletus, we find the elements operated on, and transformed by a power which he calls "the infinite," and the higher forces are gradually developed out of the lower. This, although an interesting chapter in the pro-

* Page 190. † Page 214.

gressive theory, which is probably the oldest of all theories, is not sufficiently related to the chemical characters of bodies.

After finding force and intelligence given to matter, we find Pythagoras going to quite the opposite extreme, and making, to all appearance, the origin of all things to be in numbers. Tiedemann says, "In these early days of philosophy, the abstract was not separate from the concrete, there were, it is true, different names for them, but the different meanings were not distinct.* Pythagoras took his abstract and general ideas of objects for the objects themselves, and converted general ideas into substances." Although the Pythagoreans gave much of the work of the world to fire, they had no distinct ideas on the elements.† In harmony with this kind of reasoning, they produced everything from points which seem to have been mathematical, and again, all things were produced from God, because they were produced from numbers, the first of which is "one."‡ This one is the highest God, and of Him our minds are portions.

It is a question whether we can even now in all stages separate the concrete from the abstract. The principle of the Pythagoreans is in reality a pure dynamical theory of matter where there are mathematical points and surfaces forming the limit of bodies. "All things are composed of points or unities of space which form together a number." The use of the word number introduces a difficulty into the conception of the subject, but leads us at once to the conception of force, whatever may be its origin. Their love of music led them to infuse harmony into all nature, and their love of mathematics led them to see in it order and numerical arrangement. They gave the highest place to *one;* they attached particular virtues to the small numbers up to five, apparently for mathematical reasons; they admired seven as the origin of seven chords and planets;

*Tiedemann, Vol. I., p. 96. † Page 97. ‡ Ritter, Vol. I., p. 325.

and ten the number of elementary qualities and their contraries.

On applying these numbers to matter and to calculation, we see no inclination to enter into details and to explain the constitution of different bodies by such means. Or, in other words, the numbers of Pythagoras are not of such a character as to give us the slightest clue either to the atomic theory or that of equivalents, although the remnants of his philosophy shew that he must have greatly exalted the mode of thinking wherever he taught, and led men to seek law and science or great and beautiful truths in the study of nature.

He believed in five elements. This spiritual method of derivation, or explanation, is at least an interesting instance of that mode of thought. Numbers being the origin, the monad, is a point; the dyad (or dual), is a line; the triad, is a surface; the tetrade, a geometrical body; the pentade, the physical body with sensible properties. The cube, was the earth; the pyramid, fire; the octahedron, air; the icosahedron, water; the dodecahedron, the fifth element, Aristotle's ether.

This is enough from the many contradictory notices about Pythagoras, it leads to a pantheistic view of creation, and is another feature in the progress of the subject.* The Pythagoreans spoke chiefly of morals; of nineteen writers, whose fragments I consulted, none spoke of physics.

Anaxagoras says, "The Greeks are wrong in thinking that some things are produced and others perish, for nothing is produced, and nothing perishes; but some things are mixed, some separated, some confused, some distinct, and production or perishing may be properly called composition or mixture, and decomposition or separation."

"The number of things remains always the same." He

* Tiedemann, Vol. I., p. 118.

conceives all the elements to have existed in infinitely small parts. "All things were together, infinite in number and in smallness,* and this smallness was infinite, and all being together, nothing was distinct because of its smallness." All was put in motion by a mind which governed all, and the elements attained differences of character by the preponderance of one or other. The small particles are in constant motion. There are of these particles endless numbers of every character; so a piece of gold consists of endless pieces of gold, so also silver, copper, blood, bones, flesh, water, fire, and earth, consist of infinitely small portions of these substances.† To effect this he adds, that everything was found in everything, because, as the animal grows from its food, the parts of the animal must be contained in its food. A kind of reasoning which, in one sense, is undeniable, but no proper account is taken of the formation of compound bodies, although his ideas of mixed and simple elements might have been supposed to lead him to this mode of reasoning, without the invention of *homoiomereia* (or homeomeria), the name given to this notion of every body being formed of particles like to it.

In this we find a want of discrimination in separating metals from organic substances, the changes of which seem much more allied to transformations, but we find in him philosophy obtaining a view of mind and matter as distinct, and the existence of a ruling power, God, introduced into science. But even here there was some difficulty in obtaining an idea of power without matter, and that is made like an ether, which, however, has an undefined meaning, although it seems generally to refer to a more refined kind of air, expanded into force and intelligence.

It is by no means intended to expound the various philosophies of the ancients, and so from Parmenides we can only

* Tiedemann, p. 251. † Page 1316.

find two ideas exactly suiting the subject. First, all is one. There is only one existence, and there is nothing but existence. Secondly, thought is completeness. Here we may then say that we have got an opinion different from the preceding, in which physical forces have no independent place, although some of the expressions of this philosopher would lead to believe that he considered the earth as the origin of all things, acted on by fire.

Zeno, of Elea, had four elements, warm and cold, moist and dry, corresponding to the four ordinary elements, with necessity as a moving force regulating all; concord and discord (attraction and repulsion) were some of its manifestations; but in reality nothing existed. We have then, one after another, a play on every one of the elements, each elevated in its turn, diminished to one or to a mere idea, or increased to an endless extent, where idea is only the action of an element.

Empedocles gives more distinct form to the four elements, at the same time elevating them by the name of gods. With him they are eternal, and consisting of minute parts, which although divisible, are never divided. This is an early approach to our present chemical theory of a diversity of elements, not transmutable. The principal place is given to fire. But the *four* were upheld logically, when he said they were never divided; but he afterwards adds that they were in reality only two. By him a new phase of character was given to the elements, for he says "our souls consist of all four elements, and every element is itself a soul."* "Life can only be known by life; for by the earth we know the earth, by the water the water, the divine air by the air, the devouring fire by fire, love by love only, and strife by direful strife."† The principle of love (φιλια) he held to be the origin of the elements, and the cause of their unions; the opposite prin-

* Tiedemann, Vol. I., p. 253. † Quoted by Ritter, Vol. I., p. 451.

ciple (νεικος) discord, acting with it, produced the various changes. We have here attraction and repulsion in their early days; but it is also said by him rather curiously, "discord decomposes the mixtures of the elements, and mixes fire with fire, air with air, each sort of element with its like, whilst concord acts on the contraries,"* as now found with electric + and — poles.

Leucippus first distinctly gave a meaning to the notion of small particles of bodies, which he called atoms. Democritus held the same opinions. Everything is composed of indivisible atoms. They could not be divisible, neither could they be mere points. They have neither colour, taste, smell, heat, nor cold: all these properties are given them by their various mixtures: there are various shapes. The first impulse to motion was given probably by an original force.

This is the real meaning of all he said; everything was referred to atoms, even the soul or mind itself. This is a point of great importance in the history of our knowledge of matter, and one beyond which we have not yet got in some of its relations. Here then we stand and are obliged to review our speculations and inquiries in some respect from the standing point of Leucippus and Democritus. In this view we have a distinct idea attached to the composition of bodies, and although one which might have readily come into the mind of any one who thought clearly on the subject, yet we are not aware of the difficulties attending the production of ideas, viewing them after they have been overcome. Democritus arrived at the idea of distinct atoms forming matter of every kind by the change of position. Anaxagoras was nearly at this point, but he gave the atoms characters exactly like the compound object. Democritus gives the simple bodies only shape, extension, and force.

Plato taught that the world was created by an intelligent

* Ritter, Vol. I., p. 445.

cause, and did not exist from eternity, and that it has passed from order to disorder, because order is better than disorder. God also has endowed the world with reason, it is an animated being, and not to be destroyed, but by him who made it; but he will not destroy it, as the good cannot destroy what is beautifully fitted. The world moves by its own life. The elements seem to Plato, to be only forms under which matter exists, and are convertible one into the other. The five forms of matter serve as a base to determine the elementary forms, the pyramid corresponds to fire, the cube to earth, the octahedron to air, the icosahedron to water, whilst the dodecahedron, like the sphere, comprehends like the earth, all the elements.

These are the expressions of Ritter, but on looking into the original it is not found exactly so, although the mode of reasoning somewhat justifies it. The words are "*ἔστω δὲ, κατὰ τον ὀρθὸν λόγον καὶ κατὰ τὸν εἰκότα, τὸ μὲν τῆς πυραμίδος στερεὸν γεγονος εἶδος, πυρὸς στοιχεῖον καὶ σπερμα,*" &c., as in the translation of Davies. "Let it be agreed then that according both to strict probable reasoning the solid form of pyramid is the *element and germ* of fire," &c.; another way of expressing matter dynamically.

In the Timæus he says, "First, then, that fire and earth, water and air, are bodies, is evident. * * * * We must relate, then, of what kind these most beautiful bodies were that thus came into being, and which, however unlike each other, may yet be produced from each other by dissolution. By accomplishing this we shall ascertain the truth about the generation of earth and fire, as well as those elements (water and air) which hold an intermediate position, for then we shall allow no one to assert that there are visible bodies more beautiful than these, each of which belongs to a separate class." But this does not seem to have been held firmly, because he says also at 51 Timæus, "Our plastic Creator, reflecting on all this, then mingled and united

matter, fire, and earth, gradually mixing therewith a ferment of acid and salt (ἐξ ὀξεός καὶ ἁλμυρῶν), and thus he composed a soft pulpy flesh."

As to the composition of the elements, as all was created from mind, the separation of mind and matter does not seem clear. Although he says, also, "It is evident to every one that fire, air, water, and earth are elements, but every species of body possesses solidity, and every solid must necessarily be contained by planes. Again, a base formed of a perfectly plane surface is composed from triangles. But all triangles are originally of two kinds, each of them having one angle, a right angle, and the two others acute, and one of these has an equal part of a right angle, divided by the equal sides, while in the other, two unequal parts of the right angle, are divided by the unequal sides. This, then, we lay down according both to probability and necessity, as the origin and principle of fire and all other bodies; but as for the heavenly principles thereof, those indeed are known only to the Deity, and to those among men who enjoy God's favour."* One might suppose he was speaking of the shape of the elements, and we see the want of definiteness in the forms of conception of the physical elements. Again, matter so far as it is only matter must be viewed abstractedly from all qualities; qualities are determinate conditions of matter. The motions of matter are without rule, purpose, or harmony; purpose, order, and harmony are only to be obtained from the reason.† Physics was in reality the region of uncertainty, whilst true science was in the reason: we have in modern times adopted a different opinion. This will probably be enough to enable us to find the class to which Plato belongs, as regards our subject. By going much further we get into metaphysics, or perhaps worse, into contradictions.

* Timæus, par. 28.

† Tennemann. System der Platonischen Philosophie. Vol. III., pp. 30-32.

In looking on physical nature, as far as our object is concerned, little is got in Aristotle, the idea of which did not exist elsewhere. His first form of substance, which is perceptible to the senses, is finite and perishable; he uses the ordinary four elements. There is a fifth element preceding the four elements, with a tendency neither above nor below; this is ether. The heaven is made of it, and never changes. The four elements seem to be substance united to the warm, the cold, the light, and the heavy.* He more clearly brought forward existing theories, expressing his own with greater care, and giving a history of others.

The varying phases of matter and of force shewed themselves in after philosophies. Matter rose and fell, mind rose and fell. Matter was mind, mind was matter, and even at this period we see no such nice distinction between them among the ancients as we now have, whatever be the foundation of our opinions.

The stoics said, that "matter (that is; considered in itself without quality and form) did not exist except under a certain form, and with certain properties. It is the principle of every thing which springs from it, and consequently is variable. Being absolutely passive it is infinitely divisible as body is."† But these opinions would lead into grounds too little physical.

The stoics retained the four elements which lasted so long in science, and believed that fire was condensed into air, air into water, water into earth. They called matter a collection of dimensions, length, breadth and thickness, leaving out solidity, so that matter became penetrable.‡ Only matter can do any thing. This led to the wildest assertions, that laughing, dancing, walking, crying, as well as emotions, anger, joy, fear and passions, avarice, pride and envy, vices and virtues, day, night, and sound, were bodies. This was carrying

* Tiedemann, Vol. II., p. 284. † Ritter, p. 479. ‡ Tiedemann, p. 434.

out their general principle to the utmost, and, of course, leads to an entire want of definition of natural things. Knowledge is gained by observing qualities, not by confounding them; by seeing distinctions, not by hiding them. The most contradictory assertions of all kinds have been made on the subject, especially after the great masters had done their utmost services. We must not suppose the above to be merely ridiculous. It evidently involves an extension of the idea of body, the limits of which are still unknown, and in some form or other it has often risen, and is likely again to rise, for discussion.

We find that in nearly all the cases alluded to matter has been able to put on various forms, and that it is of itself a mere abstraction. There is, in nearly all, a substratum more or less decidedly expressed. That is, a matter without properties to the senses, but capable of putting on all. The *mater*, mother of all substances. With this idea before us, most of the opinions will have some connection and rationality. Among those who denied the existence of the reality in the things perceived by the senses, we can find very little directly relating to the subject; but we must ever view with admiration and gratitude the acute minds which have done so much of the preliminary work necessary both for physics and philosophy.

The atomic system of the ancients was most fully explained by Lucretius, and leaving the nice distinctions of the stoics, and their semi-metaphysical modes of looking on matter, let us look more fully at this system than the others, as it may be said to form the beginning of the atomic theory, although the short and meagre introduction preceding may not be without interest to such as have not had time to read of the struggles of the mind in early times towards a rational expression of phenomena. It was a struggle of the most gifted minds in some of the most brilliant days of the world,

and will never cease to be an interesting chapter in man's history.

The more distinct conceptions of matter introduced by Leucippus, and promulgated by Democritus, were adopted by Epicurus, and have often gone by his name. If not ultimately the most exact, they have in many respects a practical truth, and they have the merit of having the main features clearly intelligible. The system was gaining ground at a time when the Alexandrian school was saying that matter which can be perceived emanates from the soul,* and that bodies were convertible into each other because made of one matter, which original matter had no qualities, and was capable of taking all.† This was very much in the manner of their predecessors, except that we recognise in it an increase of mysticism in their expressions. This substratum of matter has bewildered whole tribes of philosophers.

Lucretius is in the hands of every one, but read by few. The following portion on atoms is from the translation of the Rev. J. S. Watson (Bohn), with little alteration:—

"Nothing can do or suffer without bodily substance, nor, moreover, afford place (*i. e.*, for acting and suffering) except empty and vacant space. No third nature, therefore, (distinct) in itself, besides vacant space and material substance, can possibly be left in the sum of things; no third kind of being, which can at any time affect our senses, or which any one can find out by the exercise of his reason.‡ * * * * * Bodies are partly original elements of things, and partly those which are formed of a combination of those elements. But those which are elements of things no force can break, for they successfully resist all force by solidity of substance; although, perhaps, it seems difficult to believe that anything of so solid a substance can be found in nature; for the lightning of heaven passes through the walls of houses, as also noises and voices pass;

* Ritter, Vol. IV., p. 488.

† Tiedemann, Vol. III., p. 295. See also Histoire de l'ecole d'Alexandrie, par M. Jules Simon. No short sentence can give the exact truth.

‡ Book I., l. 444-449.

iron glows in the fire; rocks often burst with fervent heat; the hardness of gold, losing its firmness, is often dissolved by heat; the icy coldness of brass, overcome by flame, melts; heat and penetrable cold enter into silver, for we have felt both with our hand, when, as we held cups straight in the hand, water was poured into them from above, so that as far as these instances go there is nothing solid in nature. But because, however, right, reason, and the nature of things, compel (me to hold a different opinion,) grant me your attention until I make it plain in a few verses, that there really exist such bodies as are of a solid and eternal corporeal substance, which bodies we prove to be seeds and primary particles of things, of which the whole generated universe now consists."

"In the first place, since a two-fold nature of two things extremely dissimilar has been found to exist, viz., matter and space, in which everything is done, it must necessarily be that which exists by itself for itself, and pure (free from mixture); for wheresoever there is empty space, which we call a vacuum, there is no matter; and likewise wheresoever matter maintains itself, there by no means exists empty space. Original substances are therefore solid, and without vacuity.

"Furthermore, since in things that are produced there is empty space, solid matter must exist around it; nor can anything be proved by just argument to conceal vacuity, and to contain it within its body, unless we admit that which contains it to be a solid. But that solid can be nothing but a combination of matter, such as may have the power of keeping a vacuity enclosed. Matter, therefore, which consists of solid body may be eternal, while other substances may be dissolved (or cease to be). In addition, too, if there were no space to be vacant and unoccupied all would be solid. On the other hand, unless there were certain bodies to fill up completely the spaces which they occupy, all space which exists must be an empty word. Body, therefore, is evidently distinct from empty space.

"These bodies (which thus fill up empty space) can neither be broken in pieces by being struck with bodies externally, nor again can be decomposed by being penetrated internally; nor can they be made to yield, if attempted, by any other method, which we have demonstrated to you a little above; for neither does it seem possible

for anything to be dashed in pieces without a vacuum, nor to be broken, nor to be divided in two, by cutting; nor to admit moisture, nor moreover subtle cold, nor penetrating fire, by which all things are dissolved; and the more anything contains empty space within it, the more it yields when thoroughly tried by these means. If, therefore, the primary atoms are solid and without void, they must of necessity be eternal.

"Again, unless there had been eternal matter, all things before this time would have been utterly reduced to nothing, and whatsoever we behold would be a reproduction from nothing. But since I have shown above that nothing can be produced from nothing, and that that which has been produced can not be resolvèd into nothing, the primary elements must be a of an imperishable substance, into which every body may be dissolved, so that matter may be supplied for the reproduction of things. The primordial elements therefore are of pure solidity, nor could they otherwise, preserved as they have been for ages, repair things through the infinite space of time.

"Besides, if nature had set no limit to the destruction of things, the particles of matter would by this time have been so reduced, every former age wasting them, that no body compounded of them could, from any certain time, reach full maturity of existence. For we see that anything may be sooner broken to pieces than put together again; for which reason, that which the infinitely long duration of past time had broken into parts, disturbing and dissevering it, could never be repaired in time to come. But now, as is evident, there remains appointed a certain limit to destruction, since we see every thing recruited, and stated portions of time assigned to every thing according to its kind, in which it may be able to attain full vigour of age.

"To this is added, that although the primary particles of matter are perfectly solid, yet that all things which are formed of them, may be rendered soft, as air, water, earth, fire, because there is vacant space intermingled with the things compounded. But, on the other hand, if the primordial elements were soft, how strong flints and iron could be produced, no explanation could be given, for nature would be deprived of all possibility of commencing a foundation. The primordial elements therefore are endowed with pure solidity;

by the dense combination of which all compound bodies may be closely compacted, and exhibit powerful strength.* Moreover, if no limit has been appointed to the dissolution of bodies, there must remain certain bodies in the world which have not yet been assailed with any trial of their strength. But since (dissoluble bodies) are endued with a fragile nature, it is inconsistent to suppose that they could have lasted through an infinite course of time, harassed age after age with innumerable assaults.† * * * *

"Primordial atoms are therefore of pure solidity, which, composed of the smallest points, closely cohere, not combined of a union of any other things, but rather endowed with an eternal, simple existence, from which nature allows nothing to be broken off, or even diminished, reserving them as seeds for her productions.

"Moreover, unless there be some *least*, the smallest bodies will, individually, consist of infinite parts.‡ * * * * What therefore will be the difference between the greatest and smallest of bodies? It will not be possible that there should be any difference; for though the whole entire sum of things be infinite, yet the smallest things which exist will equally consist of infinite parts.‖

"* * * * * Those who think that fire is the original principle of things, and that the universe is maintained from fire alone, do greatly err from true reason; of which Heraclitus, as leader, first comes to the battle, celebrated for the obscurity of his language. * * * * For fools rather delight in all things which they see hid under inversions of words. * * *

"For how, I ask, could things be so various if they were produced from fire alone and pure (from mixture)? Since it would be to no purpose that hot fire should be condensed or rarefied, if the parts of fire retained the same nature which the whole of the fire still has? * * * * But if they think that fire may by any means be extinguished in condensation, and change its natural consistence, and if they shall not hesitate to allow that this may take place absolutely, then all heat, it is evident, will fall utterly to nothing, and whatever things are reproduced, will be made out of nothing. For whatever departs from its own limits, this straightway is the death of that

* l. 484-577. † Not in my copy of original. ‡ l. 603-610.
‖ l. 613-616.

which was before. Something, therefore, must necessarily remain unchanged in that fire of theirs, that all things as you see, may not utterly fall to nothing, and that the multitude of objects in the universe may not have to flourish by being reproduced from nothing.*

† "It would be to no purpose that some (of these elements) should detach themselves and depart, and be assigned to another place, and that some should have their order changed, if they all still retained the nature of fire, for whatever (fire) should produce would be in all forms only fire. But, as I think, it stands thus:—There are certain elementary bodies, whose combinations, movements, order, position, shapes, produce fire, and which, when their order is changed, change their nature; nor, as I think, are they like to fire, or to any other thing, which has the power of emitting particles to our senses, and affecting our touch by its application.

‡ "Wherefore those who have thought that fire is the primary matter of all things, and that the whole universe may originate from fire; and those who have determined that air is the first principle for the production of things; those who have imagined that water can itself form things of itself, and that the earth produces all things, and is changed into all substances of things, appear all to have wandered extremely far from the truth.

"Also those who couple the elements of things, uniting air with fire, and earth with water, and who think that from these four things all bodies proceed. * * * * § Moreover, if all things were produced from these four bodies, and all things dissolved into these bodies, how can these be called the primary elements of things, rather than, on the other hand, things (called the elements) of them, and a backward computation be made?

‖ And now let us examine the ὁμοιομέρεια (homeomeria), as the Greeks call it, of Anaxagoras, nor does the poverty of our native tongue allow us to name it in our own language. * * * He thinks that bones are produced from small and minute bones. So likewise flesh is generated from small and minute particles of flesh, and so on.

* l. 636-675 † l. 681. ‡ l. 706. § l. 764. ‖ l. 830.

" * * * * Moreover, since food augments and nourishes the body, we may understand that veins and blood, and bones, and nerves consist of heterogeneous parts. Or if they shall say, that all food is of a mixed substance, and contains in itself small elements of nerves and bones, and also veins and particles of blood, it will follow that both all solid food, and liquid itself, must be thought to consist of such heterogeneous matter, and be mixed up of bones and nerves, and veins and blood. Besides, if whatever bodies grow from the earth are previously in the earth, earth must consist of all these heterogeneous matters which spring from the earth."

He gets however into a similar difficulty, by saying that the atoms which make up these substances, have primary particles of a different figure.

* " Finally, if you think that whatever things you see in the visible world, could not have been formed without supposing the primary particles of matter to be endowed, with a nature similar to the things formed from them, your original elements of things by this hypothesis fall. For the consequences will be that you must have primary particles of matter, which, being the origin of laughter, are themselves convulsed with tremulous fits of laughter, and others which bedew their own faces and cheeks with salt tears."

As to forces, he says:—

† " For certainly neither the primary elements of things disposed themselves severally, in their own order by their own counsel or sagacious understanding; nor assuredly did they agree among themselves, what motions each should produce; but because being many, and changed in many ways, they are for an infinite (space of time) agitated, being acted upon by forces, throughout the whole, they thus by experiencing movements, and combinations of every kind, at length settle into such positions, by which means, this sum of things produced, exists."

He speaks again of their being moved of themselves, and urged by secret impulse; and gives their original motion to be a falling straight down, not in a right line. For if they

* l. 914. † l. 1020.

had fallen in a right line, there "would have been no contact produced, and no collision generated among the primary elements."

He gives a limited number of shapes to the atoms, but sees no need of hooks to keep them together. This variation in shape leads also to a variation in size, but the atoms themselves are infinitely numerous.

Primordial atoms are not sentient, or they would "produce nothing but a crowd and multitude of animals." *

So far Lucretius. This theory of the constitution of material substances requires us only to conceive of one class of substances: for although some are larger than others and differently shaped, that is not a necessary supposition. It is the atomic theory properly so called. Forces are as much left out as possible.

One of the most complete atomic systems seems to have been produced in Hindostan. There it is said matter consists of the smallest possible bodies which are indivisible. We must at last arrive at something limited, otherwise the smallest, as well as the greatest, would be infinite. The first compound is binary, the union of two being the simplest, then there is a formation of three binary atoms, and a new compound of four quaternary atoms, and so on. The atom is equal in size to the sixth part of a particle seen by the sun's rays. A superior force draws the atoms to each other. The union is not a mere juxta-position, but one drawn by a particular affinity.

This is a peculiar mode of combining the atoms, we have not three or four simple as we might expect. It is interesting to find exactly the same course of reasoning about natural things among the Hindoos as in Greece, although Mill, in his history of India, ridicules it as the wanderings of the mind; the same might be said of the Greek ideas. We find there,

* Book II., l. 917.

that "from intellect arose fire; from ether, air; from air, fire and light; from light, a change being effected, comes water with the quality of taste; and from water is deposited earth with the quality of smell." To fire is attributed the quality of figure: Mr. Mill supposes light is meant; to air the quality of touch; to ether the quality of conveying sound. Mr. Mill thinks *hearing* is meant. The qualities are confused as in that Greek system, which said, fire only can understand fire; air only can understand air; and so on.* The Hindoos, also, had their four and their five elements; their eternal elements; and the elements proceeding from the will of God, to cease when he pleases. They, too, had a system which made mind a substance and the affections subtle bodies.† But they did not always confuse it, as is seen in Menu. "He having willed to produce various beings from his own divine substance, first with a thought created the world." On the constitution of matter we see them speaking as plainly as the Greeks; and now on the philosophy of the combination, we see an instance of still greater farsight. We can readily believe that the following is very beautiful in the original poetry. There is a poetical beauty even in the prose, which makes it dance like the power it describes in spite of the gravity and intellectual nature of the subject.

From the poem of *Shi'ri'n* and *Ferha'd;* or *the Divine Spirit and a Human Soul Disinterestedly Pious.*‡ "There is a strong propensity which dances through every atom, and attracts the minutest particle to some peculiar object; search this universe from its base to its summit, from fire to air, from water to earth, from all below the moon to all above the celestial spheres, and thou wilt not find a corpuscle destitute of that natural attractability; the very point of the first

* History of British India. By James Mill, Esq., 1848. Vol II., pp. 94-95.

† Colebrooke, Asiatic Researches. Vol. IX., &c.

‡ The Works of Sir William Jones, 1799. Vol. I., pp. 170-171.

thread, in this apparently entangled skein, is no other than such a principle of attraction, and all principles besides are void of a real basis; from such a propensity arises every motion perceived in heavenly or in terrestrial bodies; it is a disposition to be attracted, which taught hard-steel to rush from its place and rivet itself on the magnet; it is the same disposition, which impels the light straw to attach itself to the amber; it is the quality, which gives every substance in nature a tendency toward another, and an inclination forcibly directed to a determinate point."

CHAPTER V.

FROM LUCRETIUS TILL THE DECAY OF ALCHEMY.

EGYPT seems to have attained to a knowledge of the qualities of matter never attained in the Greek and Roman empire, and it is highly probable that a more correct theory existed in conformity with the more advanced practice. But it concealed its knowledge of science as it concealed its history, and the treatises which profess an Egyptian origin are more unintelligible than hieroglyphics. One is at first inclined to believe that the mystic mode of writing was simply a result of ignorance, because when the Alexandrian school begins to philosophize, it uses language as clear as it is capable of obtaining, although prevented from great clearness by the mystic nature of its region of thought. But the results of the arts exist to tell us that in the region of actual knowledge they were no triflers, whilst in that of speculation they made such efforts to gain a knowledge of things superhuman, that if they have failed it is not for want of devotion, truth, and energy.

We have seen matter viewed in various aspects according to the philosophies of the time, and we might almost have added according to the speculations, viz., those of the popular beliefs and the mythologies of the time, when substance was strangely mingled with spirit, and when the gods of the woods were scarcely separated from the woods themselves. Such a view is analogous to the ancient Scandinavian method of looking at creation, when they formed the earth from the body of Ymir, and the sea from his blood.* It shews us that the intellect does not readily conceive of mind and matter, of

* Prose Edda: Mallet's Northern Antiquities.

force and substance, as distinct; that in fact the early mind looks on matter in an abstract sense as a thing made of qualities; which qualities may change indefinitely, and all things easily be transformable. The elements in this philosophy are not elements of science, but the common elements of every day life; in other words those aggregate objects with which nature has made the world, without reference to their mechanical or chemical disunion. In reading the ancient authors, one is disposed to think that this was the general sense in nearly every system, and that a strict meaning was not attained by any one of them, except the atomists. In vain do we quote their opinions, another quotation comes with a meaning in exact contradiction, and the vague and indefinite is the only final result. It is not to be wondered at that this should occur with the Alexandrian school, which neglected the body, despised the world, and sought truth only in that state of mind called ecstasy, which, however exalted it may be, is very naturally shunned by us as a dangerous forerunner of the loss of reason, or as a state of hallucination. With them, there are four elements, it is true, but these are like the elements of Plato and others, they are convertible, and there is an abstract matter from which all things are made by the putting on of various properties. It is the origin of all things that exist, and has the power of becoming everything, but it exists only as a power. This leads into metaphysics, which I avoid; for us, it is enough that they looked on the four elements as transformable, which have, therefore, not the character of elements. What are they, then? They are nothing real, the qualities are changeable, but there are no data given for finding whence these qualities come, and so we are led into a region of mere dreams, and nature is a power playing upon us every imaginable illusion by means of its forces; "all which forces have their origin in *one*, because unity is the basis of all things." Here, then, being no begin-

ning of matter, properly so called, it is in vain to look for a beginning of such elements as cannot be convertible, even if they had not expressly stated that all are capable of transformation.

The early stages of chemistry and metaphysics have unconsciously met. From Alexandria, with its spiritual or mystic views, the *ars sacra*, ἱερα τεχνη, came, perhaps carried by some who had little to teach, at least speaking of matter in terms too mystical to bear to us any distinct meaning, and holding their knowledge too sacred to be given to ordinary mortals.

The spiritual faculty was becoming developed in man. It had existed before that time in its greatest powers in individuals, but it had not as yet educated nations. Christianity had directed man to the consideration of God and the moral part of our nature with such power and success, that philosophy was carried away by the current, and at length became entirely absorbed. But to philosophize on morals and upon spiritual phenomena, so to speak, is to become mystic, unless great care be used; and this current of thought induced, acted a long and important part in the history of the world. The course of this philosophy, for many ages, seldom led it to touch on matter with any firmness of step. Man lived in his thoughts, still fancying himself capable of living independent of mere external things, his spirit still disinclined to believe in the great power which the base creations under his feet were capable of exercising over his finest feelings. There was a struggle against matter, one which cost thousands of victims, buried or slain in the ranks of asceticism, or made useless by the perverted consciences which they carried with them into common life. Matter was not seriously believed in, it was a strange power, a thing capable of every action, acted on by the Deity, or by the spirits of those who either in this world, or the next, had obtained a share of the primeval influence of the Creator. With such men there could be no true philosophy

of matter. This mystic mode of philosophizing had no doubt been the mode adopted by the chemists of the earliest centuries, but little or nothing now remains. The following is from Zosimus, the Panopolitan, upon the "Divine water:"—

"The mystery sought is great and divine, for all is from it, and by it. There are two natures and one substance. The one carries off and subdues the other. This is silver-water (mercury), the male-female principle always escaping, constant in its properties, the divine water unknown to the world, the nature of which is inexplicable. For it is not a metal, nor is it water; it is always in movement, nor a body; it is all in all; it has life and spirit, and may be held." *

This is quite in the mystic style of the neoplatonic philosophy, although the tone is evidently lower than that taken by the philosophical mystics themselves. We see in it clearly the style of the alchemists, and the impulse that was given to the study of alchemy seems to have come from this source mainly. The philosophy of Aristotle having for many ages taken the lead, the four elements followed him, and we find them acting an important part in nearly all alchemical treatises, but the mode of reasoning is by no means Aristotelian, and is in every sense mystic. And why mystic? This word is itself of indefinite meaning, but I would say that the reasoning on physical law is mystic, when there is no distinction made between the laws of material nature and those of the mind of man, when there is a confusion between that which is done by natural law and that which is caused by the spirit of man, during his observation of the phenomena; when, for example, an experimenter must suit his frame of mind to the experiment, or find that nature will refuse to act. It may be said that such a theory involves the continual presence of God, supporting constantly his own laws, as in a Hindoo system, † or it is a theism which makes God everywhere acting, not

* Hoefer. Historie de la Chimie, Vol. i., p. 259.

† Blakey's History of Moral Science.

according to fixed laws, but according to man's deservings. The latter was not much carried out, and the first corresponds more closely to the general opinions of these writers. It is a kind of physical mysticism also to confuse elements with each other, and powers with substances, a state corresponding exactly with the mysticism of a more mental character, which confounds the Deity with his creatures, and desires to absorb embodied man in his unembodied creator.*

The four elements were preserved amongst the Arabians, and their corresponding qualities, hot, cold, dry, and moist, and the most wonderful power was given to a mixture of them all; it was in this way that "*Hai Ebn Yokdan* was produced without father and mother; it chanced that a certain mass of earth was so fermented in some period of years, that the four qualities, viz., *hot*, *cold*, *dry*, *moist*, were so equally mixed, that none of them prevailed over the other."† This was Avicenna's opinion of what was possible.

Fermentation was a favourite method of explaining difficult phenomena, and is now. As it has now been to a great extent explained, the lovers of the occult will be obliged to seek deeper for their theories.

We may find then another origin for the mystic mode of viewing matter, as well as for the four elements, as far east as Hindostan, from which the Arabians may have brought it; but we cannot, with historical certainty, trace it to its home; and it is possible that these two origins, Asiatic and Egyptian, may have been originally the same. But what is clear to me is, that the style of the alchemist had the same origin as the

* Whilst this Memoir was being printed, I obtained the "Hours with the Mystics," by Robert Alfred Vaughan, B.A., who says, vol. 1, p. 26; "mysticism, whether in religion or philosophy, is that form of error which mistakes for a divine manifestation the operations of a merely human faculty." This has no doubt influenced the expressions used above. Hoefer has observed the connection between the religious writers of early centuries, but has not traced it up.

† The Improvement of Human Reason, exhibited in the Life of *Hai Ebn Yokdan, by Abu Jaafar Ebn Tophail.* Translated by Simon Ockley, A.M.

mystic style introduced into religion and philosophy. The same condition of mind would produce both, according to the subject to which it was applied, and that condition seems to have a historical as well as natural connection.

Albertus Magnus followed closely the reasoning of Avicenna. He was born in 1193, a few years earlier than Roger Bacon. Some of his opinions on matter shew distinctly their origin. He says,* "That matter and power are the principles of each body is clear from the reasoning; for having taken away all the accidental forms, we arrive at length at a substantial form, which being removed by the intellect, there remains a something very occult, which is the first matter." (Tandem venitur ad formam substantialem qua adhuc abstracta per intellectum; remanet quoddam valde occultum quod est prima materia.) He says also, cap. iii., "Matter has a natural appetite for form." He discusses whether the metals are generated, and decides by a peculiar reasoning that the *materia prima* is not generated, but created, "because if it were generated it would be from some other matter, therefore matter would contain other matter, and so on without end; therefore the *first matter* is not generated, but created. Creation means to make out of nothing." But the elements may be generated, and Albertus Magnus easily changes one into the other. "The generation of one is the corruption of the other, and *e converso*. From the generation of one then follows the corruption of the other."†

That the four elements held their ground we see from no one more clearly than Roger Bacon, born 1214. We see there, too, the opinion that the original matter, *yle*, has none of the qualities of a body, but is matter in the abstract. His words are:—

"Elementa sunt quatuor, ignis, aqua, aer, terra, modi id est

* In the portion of his work on natural philosophy, entitled "Physicorum," cap. 2.

† His chapter, *De Generatione Elementorum*.

proprietates, sunt quatuor, calor, frigiditas, siccitas et humiditas; et yle est res in qua non est calor, nec frigiditas, nec siccitas, nec humiditas et non est corpus. Et Elementa sunt facta de yle; et unumquodque elementorum convertitur in naturam alterius elementi et omnis res in quamlibet. Nam hordeum est equus per vim, id est, naturam occultam; et triticum est homo per vim, et homo est triticum per vim."*

"There are four elements, fire, water, air, and earth, that is the properties of their condition are four, heat, cold, dryness, and wetness, and yle (the true matter) contains no heat, nor cold, nor dryness, nor wetness. The elements are made of yle, and each of the elements is converted into the nature of the other element, and everything into anything else. For barley is a horse by possibility, that is, occult nature, and wheat is a possible man, and man is possible wheat."†

This explanation is exceedingly clear and rational, and founded on a good deal of observation. We see here in Roger Bacon's ideas, which are truly in the spirit of the ancient philosophers, the atomic being excepted, that all being might arise from this source, called *yle*. There is, however, an improvement in the mode of expression; we understand perfectly what he means, and there is a terseness seldom, if ever attained, either by those before him, or after him. It separates him from the mystics, properly so called, although there is the easy passage of one element into the other, and the unsubstantial principle from which all are made.

To this scholastic form the mystic method attached itself, coming apparently with the Arabian learning into Europe, as well as by direct transmission through Greece and Rome, having lived for some centuries with little growth, and destined to be led with more vigor towards its extremes by the reviving intellect of the West.

* De Arte Chymiae. † This yle is the Greek ὑλη, matter.

The method of viewing matter being sufficiently confused by any one of the systems, was not less so when they were united, or rather, I may say, carelessly mixed and confounded. When we add to them an infusion not only of the moral and religious method, but of morals and religion themselves, as elements in the production of results, we have a system of nature which all minds refuse to contemplate with patience, and on which our imagination refuses long to rest, so bewildering is it to the one, and so wanting in beauty to the other. But in a history of man it will always form an important chapter; as a dissection of the mind it will always have a psychological value, and as a portion of the progress of physics it will never cease to be worth preserving for at least one lesson to the student.

The Arabian chemists who took the lead in introducing it, acknowledge Geber to be their master, and he composes metals of sulphur and mercury. At the same time he says, "We see no ox transformed into a goat, nor any one species transmuted into another, or by any other artifice so reduced. Therefore, seeing metals differ in themselves, can you transform one into another, according to its species, or of such a species make such a species? This seems to us sufficiently absurd, and remote from the verity of natural principles. For nature perfects metals in a thousand years; but how can you, in your artifice of transmutation, live a thousand years, seeing you are scarcely able to extend your life to a hundred?" He considers the work as done by the stars, which cause the generation and corruption, but being ignorant of their power, we cannot use it. "Likewise, also in things natural, this is the order; it is easier to destroy them than to make them. But we can scarcely destroy gold, how then can we presume to fabricate the same?"

He was, therefore, not a gold maker, although holding the abstract theory of the possibility of transmuting. His followers did not follow him in this, and whilst quoting his

authority, probably knew nothing of his more rational practice; they evidently retrograded, but it was probably needful, in order to satisfy themselves.

A few specimens of the Stoechiometry of the period onward to Boyle will be needful to explain to what a woeful state the science fell; or shall we say rather what a dreadful struggle it had to come to the light; or shall we say that it was only in analogy with the general opinions of the times, and a picture of the general state of mind? For the honor of human nature I would prefer to say that the highest minds being engaged in the cultivation of the more spiritual faculties, in the development of the moral nature of man, in struggling for freedom of body and of mind; the lower ones were engaged with science, and lived a miserable existence, fed by the crusts from the table of the brighter intellects. We may add also that science was, with many persons, as now, a mode of making money only; and many who had no love for it, joined in the pursuit, and are now by us apt to be confounded with true men. This explains many, but not all the cases.

Science and religion have always influenced each other, and it is interesting to trace them. In the *Tractatus aureus*, by a German *philosopher*, the piety is seen. In making experiments, he says to *every pious God-fearing chemist:* "Above all it is needful to be pious, to lift the heart to Him, with true, ardent, and not doubtful prayer, and to ask the gift from Him only."* Again, of the *Quinta essentia.* "It is the universal and scintillating fire of the light of nature, which has the celestial spirit within it animated in the beginning by God, and penetrating all things, called therefore, by Avicenna, the spirit of the world. For as the soul is found in all the members of the human body, and moves itself, so this spirit is found in all the elementary

* Museum Hermeticum, page 80.

creations, which is also the indissoluble connection of body and soul, and so is a most pure and noble essence, full of wonderful efficacy and virtue, in which all mysteries lie hid." *

What this was we see better in the next quotation from Mehung. "Now I consider you informed of this valuable truth, that there is no element which is not worked into another, so that operating on one, the other is operated on. For example, fire is worked into air and earth, if fire excites the operation. The earth is the mother and sustainer of all things; as all things under heaven which are subject to putrefaction, are produced by birth in the warmth of its womb. Only the power of God allows me to return the four elements again into the fifth essence, and that is called the *prima materia*, which is mixed generically in every element." † (Quae in uno quolibet elemento generice mixta est.)

The idea that all things grew was a common one to them, as well as to Dr. Johnson, of modern times, and of great learning. That every thing grew from seeds, is said to be the most ancient of doctrines.‡ There is no detail as to whence the increase of volume came; growing was the cause and the explanation. As it is said in "*The way of Truth*," "so long as you boil, so long you putrefy it, and the substance is exposed to putrefaction, like corn which is thrown into the earth, and which is preserved in the earth itself by the heat of the sun, but must putrefy by natural rain before anything new will grow from it." ‖

And Basil Valentine, "on the philosopher's stone," says, "the vivifying power of the earth produces all things which spring from it, and he who says that the earth is without life, speaks contrary to truth. For the dead can supply nothing to the living, and neither can the dead grow, because the

* Museum Hermeticum, p. 84.
† Id., p. 151. Demonstratio Naturae. By Mehung.
‡ Aristotle, quoted by Ritter, Vol. i., p. 180.
‖ Idem p. 186.

spirit of life has fled; therefore the spirit is the life and soul of the earth, it dwells in it, and is operated on from celestial and sidereal, into terrestrial; for all herbs, trees, and roots, as well as metals and minerals, receive their strength, increase, and nourishment from the spirit of the earth."* Although this leads to conclusions beyond the known facts, it is a style of reasoning not to be entirely found fault with, and much less can it be called irrational. It is a kind of middle path between calling earth an animal and our present opinions; but Basil Valentine showed intellectual vigor, and formed an epoch in his science. He was born about 1413.

Raymund Lully, who takes us back again to the age of Albertus Magnus and Roger Bacon, was universally quoted by alchemists as a great master, he was born about 1235. He writes;

"Matter says I am a being from which something is made by *passionhood*,† and that substantially and accidentally, because I am the leader, since from me who am the primitive is made that particular matter which is the substantial part of substance, as the matter of a rose, of a horse, and so on.—God is my end and cause, and I am simply his effect.—

"Matter says, I am absolute passionhood under absolute form to which I am conjoined, and as all the river waters are derived from the sea and return to it, so from me are derived all matters in particular because I am absolute."

"Again matter says, I am not a being absolutely existing, potentially so; because, if so, the subject in which I am would be sustained potentially, and so successively *ad infinitum*, which is impossible. I am, therefore, a being existing potentially to all particular substances existing under particular forms."

"In my nature there is not found form, which is from me

* Museum Hermeticum, p. 403.

† I imagine this word to imply better than any I know the state of ready excitability to impressions.

and on account of me, because if so I should not be absolute *passionhood* and my parts would be deprived by me of essence and nature, which is impossible since I am simply absolute."

"Matter says, I am individualised by quantity, as when I am so much; either long, broad, or deep; either round, in a circle, or spherical body; or qualified, for example, when shining or warmed in flame and reddened in red wine, sweetened in honey, heavy in earth, or become light in the fire."

Some may prefer the original of this :—

Ait materia : ego sum ens ex quo fit aliquid passionando et hoc substantialiter et accidentaliter, quià dux sum, quoniam ex me quae sum primitiva fit materia particularis quae est substantialis pars substantiae. * * * * *

Ipse (Deus) est meus finis, mea causa prima, et ego sum effectus simpliciter ejus.

Ait materia, sum absoluta passio sub absoluta forma sub qua sum conjuncta et sicut ex mari derivantur omnes aquae fluviales et ad istud revertuntur, sic a me derivantur omnes materiae particulares et ad me revertuntur, quià absoluta sum. Rursus ait materia. Non sum ens existens in potentia absolutè, quià si sic subjectum in quo essem, sustentata esset in potentia et sic successivé in infinitum quod est impossibile. Sum ergo ens existens in potentia ad omnes substantias particulares sub formis particularibus existentes.

In mea matura non invenitur forma quae sit ex me atque propter me, quià si sic, non essem passio absoluta et secundum quid meae partes essent à me ab essentia et natura privatae, quod est impossibile, cum sim simpliciter absoluta.

Ait materia. Individuata sum per quantitatem cum quae sum quanta, ut puta longa, lata, et profunda, aut in circulo vel in corpore sphaerico rotunda : sum qualificata, ut puta in flamma lucefacta, calefacta ; et in vino rubro rubefacta, in melle dulcificata, et in terra ponderosa ; et in igne levificata.*

He recognises the four elements, and believes them to exist

* Raymundi Lullii Opera—Philosophiae Principia—De Materia. Strasburg, 1609. I suppose more than one mistake to occur in the Latin. I may be wrong, but it is quoted correctly.

in all things *elemented.* We see in him great acuteness of thought and much valuable observation, and need not wonder that he was a great master among succeeding alchemists. We see in him the Greek mode of reasoning on the abstract principle of matter, but the word which was intended only for a general name, comprehending all substances, is taken for an existence by itself. I know that there are other means of getting into the same difficulty, but we find the alchemists haunted by the ghost of the fine abstractions of Plato's divine ideas of things existing on earth, and the belief in abstract matter, and not seeing the intellectual origin they seek to find them out in substance, and so we have a search for the *prima materia,* which orginally had no power of being handled, and scarcely of being conceived.

They tried in vain to hunt the ghost down, and to handle it in their fingers. Whilst the intellect was unable to grasp the idea, they actually sought to catch it in a bottle. This misconception, so singularly ludicrous, makes me suppose that the class of minds generally engaged in chemistry were inferior, but it may be enough to suppose that the mistake once made was not easily rectified. It seems to me to be without doubt that this is the origin of the singular alchemistic chase. If "immaterial substance" was a "philosophical imposture in philosophy," as Coward called it last century, how much more has it been to alchemy. Yet, I know a living and *intelligent* man who searches for the *prima materia.* It may truly be said that the world is governed by ideas.

This *matter* having become on one side a mere substance, it is not to be wondered at that on the other side it became entirely the opposite, and was a representative of abstract force, as we may see in expressions of Plotinus, and others of the time, which it would take too much time to quote.

Again, we have a search for the fifth essence, which contained all things and was made up of all things. Here is an evident proof of a decrease both of intellectual power and

of knowledge, and an illustration of the danger of a little learning.

This *prima materia* was even supposed by some to be the substance on which it was needful to begin to operate, so far had it descended. Flamel says, "Having thus obtained this delicate and precious book, I did nothing else day and night but study upon it, conceiving very well all the operations it pointed forth, but wholly ignorant of the *prima materia* with which I should begin, which made me sad and discontented." * Although this, as is afterwards shewn, was a matter of preparation also, the point of departure of the second process.

Artephius, who lived in the twelfth century, and was born, according to his own account, in the second, and wrote a book on prolonging life at the age of 1052! may be quoted as a true mystic chemist, shewing us, too, by the numerous names that he gives to this liquid, that he even at that time inherited it from a long ancestry. "Our dissolving matter, therefore, carries with it a great tincture, and a great melting or dissolving; because that when it feels the vulgar fire, if there be in it the pure or fine bodies of sol or luna, it immediately melts them, and converts them into its white substance, such as itself is, and gives to the body colour, weight, and tincture. In it also is a power of liquefying or melting all things that can be melted or dissolved; it is a water ponderous, viscous, precious, and worthy to be esteemed, resolving all crude bodies, into their *prima materia*, or first matter, viz., into earth and a viscous powder, that is into sulphur and mercury." The indefinite notion of the origin of properties we see, when he says, "the property, therefore, of our water is, that it melts or dissolves gold and silver, and increases their native tincture or colour; for it changes their bodies

* The Lives of the adepts in Alchemystical Philosophy, with a Critical Catalogue of the Books in this Science, and a Selection of the most celebrated Treatises on the Theory and Practice of the Hermetic Art. Page 34.

from being corporeal, into a spirituality, and it is this water which turns the bodies or corporeal substance into a white vapour, which is a soul that is whiteness itself, subtle, hot, and full of fire."

Here is an attempt to separate chemical action from mechanical. "It appears then that this composition is not a work of the hands, but a change of the natures; because nature dissolves and joins itself, sublimes and lifts itself up." The nature of the treatises which he studied is seen in a quotation of his own. "It is also the most acrid vinegar, concerning which an ancient philosopher has said, I besought the Lord and he shewed me a pure clear water, which I knew to be the pure vinegar, altering, penetrating, and digesting. I say a penetrating vinegar, and the moving instrument for putrefying, resolving and reducing gold or silver into their *prima materia* or first matter." *

That the chief aim was this originally metaphysical idea of matter, we see also as late as 1691, in the Second Aphorism of "Baron Urbiger," whose more modern style has helped him to a more condensed expression. "An indeterminate matter being the beginning of all metals and minerals, it follows, that as soon as any one shall be so happy as to know and conceive it, he shall easily comprehend also their natures, qualities, and properties." "It is found every where, at all times, and only by our science."

A favourite mode of representing chemical action, was by the analogies in the growing of plants and animals. Zosimus says it is the (Hydrargyrum) water-silver, the male-female principle, the principle always escaping constant in its properties. The same idea proceeded forward, and in 1409, Nicholas Flamel says, "minerals taken out of the earth may be changed if beforehand they be spiritualized, and reduced into their sulphureous and argent-vive nature, which are the

* Idem. The Secret Book of Artephius.

two sperms, composed of the elements, the one masculine, the other feminine. The male sulphur is nothing but fire and air; and the true sulphur is as a fire, but not the vulgar, which contains no metallic substance. The feminine sperm is argent-vive, which is nothing but earth and water," &c.

And Norton, about the same time, reasons thus;

"Metalls of kinde grow lowe under grounde,
For above erth rust in them is found;
Soe above erth appeareth corruption
Of metalls, and in long tyme destruction,
Whereof noe cause is found in this case,
But that above erth thei be not in their place." *

But he denies, like Geber, any growing in glass vessels;

"For cause efficient of mettalls find ye shall,
Only to be the vertue minerall,
Which in everie erth is not found,
But in certain places of eligible ground."

When once grown, however, they could not multiply according to him;

"Trewly ye maie trust as I said before,
How of one ounce of silver, maie silver be no more."

But this was not the general opinion, as they were supposed to grow in the preparation, as Ripley says; On calcination, v. 15; †

"If thou intend therefore to make
Gold and sylver by craft of our philosophy;
Thereto nother eggs nor blood thou take,
But gold and sylver which naturally,
Calcyned wysely, and not manually,
And new generation wyll forth bryng,
Incresying theyr kynde as doth each thyng."

As metals grew by their life, and life is the blood, so it was a desirable thing to find out the blood of metals;

"The true blood of mettalls is hard to have." ‡

As the *prima materia* was of indeterminate properties, was

* Ashmole's "Theatrum Chemicum," 1652, pp. 18, 19, and 20.

† Same, p. 132.

‡ "Anon." Same, p. 406.

in fact originally only an intellectual conception, come to in the consideration of matter, abstracted from its apparently unnecessary properties, and converted into a reality by the alchemists, so they, in their turn, converted their elements, which are partly the usual four, partly sulphur and mercury, or with salt added, into mere abstract ideas. Their sulphur was an ideal sulphur, and Paracelsus gave every body its own peculiar sulphur and other elements. This was a step towards a recognition of many distinct indestructible bodies, and a progress out of the purely mystical consideration of the subject.

The opinions held generally by alchemists, having arisen, as I think, from the spiritual and religious state of man, more than his directly intellectual, it would require more space than can be given here for a history such as would suit this view of the case. The early Greek manuscripts, mentioned by Olaus Borrichius, and partly given by Hoefer, show the inclination there was to write with, or to proceed from, the current religious opinions. Isis and Osiris were important alchemistic names from the Alexandrian school, and salt, sulphur, and mercury, have been connected with the trinity of Christians.

It is not the place here to follow the detailed opinions of the alchemists, they are taken rather in the mass, and twelve or fourteen hundred years of their opinions thrown with little order together. This I see no great reason to alter, the men differed so little in their radical opinions. Salt, sulphur, and mercury, were sometimes the origin of the metals, sometimes of all things, each had its own spiritualization, each was the greatest or the least in its turn; and as a curious instance of recurrence of opinion, we find Palissy, in the 16th century, a man full of shrewdness of observation, saying, as Thales did so long before, "that the commencement and origin of all natural things is water." * But then he believes in more

* p. 217. Oeuvres Complètes de Bernard Palissy. Edition par Paul Antoine Cap, 1844.

than one kind of water, one of which he considers a fifth element. This, however, has to do with the history of the idea of liquid and fluid, which must not be entered on here. In saying this, Palissy was reasoning from the state of metals in solution, for he believes they grew, and were found in common water. As he says;

"Il (le createur) a commandé à nature de trauailler, produire et engendrer, consommer et dissiper; comme tu vois que le feu consomme plusieurs choses, aussi il nourrit et soustient plusieurs choses; les eaux debordees dissipent et gastent plusieurs choses, et toutefois sans elles nulle chose ne pourroit dire ie suis. Et tout ainsi que l'eau et le feu dissipent d'vne part, ils engendrent et produisent d' autre. Suyuant quoy ie ne puis dire autre chose des metaux, sinon que la matiere d' iceux est vn sel dissoult et liquifié parmy les eaux communes, lequel sel est inconneu aux hommes; d'autant qu' iceluy estant entremeslé parmi les eaux, estant de la mesme couleur que les eaux liquides et diafanes on transparentes, il est indistinguible et inconnu a tous; n' ayant aucun signe apparent, par lequel les hommes le puissent distinguer d' auec les eaux communes."*

Matter then appeared to the alchemists as it has done to the greater part of early thinkers, and to most of those who do not think, as a power subtle and changeable, capable of every transformation, and dependent on laws by some held eternal and immutable, by others dependent on the spiritual condition of man. We have no distinct clue in such opinions to any definite ideas about composition.

* Same, p. 194. In his objection to alchemy, Palissy has not shone so much as in his pottery, and even in his other scientific inquiries. Bötticher, also a potter, a less honourable and less able man, has been, by his discoveries in Dresden porcelain, the occasion of a saying suitable to the times, and shewing the true value of the alchemists, viz.; their accidental discoveries in the arts. When his gold making failed, and he made porcelain, it was said—

O Gott Du grosser Schoepfer,
Aus einem goldmacher wird ein Toepfer.

Ye heavens, alchemy has win my votes,
A goldmaker's changed to a maker of pots.

p. 303. Hist. Crit. Untersuchung der Alchemie. By Wiegleb, 1791.

On the four elements we have occasionally opinions which may be called definite, as when they are said to form various combinations with various properties, but these are held with no tenacity. In the salt, sulphur, and mercury, we have a clue to definite composition, but we have seen them spiritualized and held with still less tenacity. In the elements of Leucippus, Democritus, and Lucretius, we have a definite view taken of the subject, but that was soon neglected, to be revived in part when alchemy was falling. We have had an approach to a distinct view before us in that of forces, or unextended points, forming the origin of matter, brought forward later, and, perhaps, in an entirely original form, but at least more detailed, by Boscovich.

CHAPTER VI.

OPINIONS DURING THE TRANSITION FROM ALCHEMY TO CHEMISTRY.

The ages so rapidly passed over comprehend two thousand years. The attainment in chemical science has been as yet small. All the theories have been abstract; they have been efforts of the mind to comprehend matter, with a very meagre, if any, classification of phenomena. We might ask ourselves if this was caused by the scarceness of facts, or by the want of a mind to perceive them. As to scarceness of information this may be doubted, numerous truly chemical arts had been from early ages in use; many had fallen into disuse. The observers that now come to the science are not so much characterized by the multitude of their discoveries, as by the minuteness of their observations, and the penetrating nature of their reasonings. The progress of mankind has often been compared with the progress of individuals, and the analogy serves here to point out a cause. In the history of every mind, especially of the mind of the student, there are seasons where facts are collected and books read with diligence, theories formed in great numbers, reasonings adopted and thrown aside; opinions are heaped up, but no distinct opinion is formed, and a vague stare into the difficulties before it is the only result of the labour, for knowledge it can scarcely be called, where nothing is well arranged, and nothing is actually known. The imagination then takes the place of the helpless reason. A poetical mind may find in this state a field for its highest powers, a weaker mind will seek explanations more or less mystical, and a still weaker mind will be contented with the merely mysterious. But a vigorous mind, in which reasoning and observation predominate, is found gradually to

collect from the disorderly information, certain impressions, which, in time, become more distinct, until at last an idea is obtained which can be laid hold of. Whether this idea be false or true, is of little consequence; it is in any case enough to give order to the museum of his mind, and for the first time that mind may be said to be informed. Every thing is examined under the influence of this idea, which may change constantly, as it is constantly seen to be unsuited to the facts, but every change may be a progress; and even if from false to false, is nevertheless a constant gain, so much false being left behind. The dreaming age of chemistry had lasted long; the minds occupied had been satisfied either with the more poetical observation of nature, or with the mystic and mysterious; an idea had entered into the mind of the chemists of the age, that some exact explanation could be obtained, and so we find them hunting it down from point to point with acuteness, energy, and hope, ever increasing as the object seemed rapidly to be approached. But how was this idea first attained? How does the first idea rise in the mind of the individual enabling him to see order and beauty in all the shapeless learning that he had been amassing? It is a progress of mind which is not for us to discuss here. The requisite for it is intellectual energy, and how that has been aroused in modern Europe, many great writers have done their best to show. It was by a combination of many great causes, natural stages in the edueation of the species. We are surprised to find that the finest writers of the world should have existed when thinkers in science were scarcely at the rudiments; but we see constantly that the highest literary powers may be unfit to comprehend any scientific truth firmly, and we see, even among the scientific world, that some branches have still scarcely begun their independent life. The world cultivated poetry and eloquence, and attained the highest stage that we know, when no law in the mixed sciences could be rigidly and certainly stated. The writers, quoted in this chapter,

go over again much of the same theoretical ground traversed before, but frequently with much less philosophical grasp than the ancients, and much less largeness of conception, although they frequently gain by what seems the littleness of their ideas, and the smallness of their aims. They are contented to speculate on an acid, instead of the formation of a world, whilst those who take a wider ground, such as Newton and Boscovich, can only be said, as far as our subject is concerned, to reproduce and improve earlier philosophies.

The tendency from this time is to increase the number of bodies, which, if not at first called elements, are at least treated as such. This is a needful step towards the chemical theory of matter in its present stage.

Glauber, although a believer in the general transmutability of substances, did much to help forward the notion of distinct elements in his observations on the affinity of bodies. He explains the evolution of ammoniacal gas from sal ammoniac by a fixed alkali, showing that he understood well combination and decomposition, the stumbling-block of so many, and the introduction to definite compounds. "But that a spirit is distilled off by the addition of fixed salts; the reason is that fixed salts are contrary to acid salts, and if they get the upper hand do kill the same, and rob them of their strength, whereby those things which are mixed with them are freed from their bond, and so it falls out here with salt armoniack, that when by addition of à vegetable fixed salt, the acidity of the salt armoniack is killed; the salt of urine, which formerly was bound therewith, gets its former freedom and strength, and being sublimed turns into a spirit." *

Boyle, who attacked alchemy, and may almost be said to have begun modern chemistry, or rather let us say the transition period, seems to have had one of the clearest, most straightforward, and most common-sense methods of viewing

* Page 49. The Works of the Highly Experienced and Famous Chymist, John Rudolph Glauber. London, 1639.

phenomena, of any of his period. One feels, on reading his works, that on another subject he might have written so as to be even now and at all times read with delight, whereas, in the dangerous and difficult fields of chemistry, he has only left matter serving as landmarks, to shew us the way in which the mind has been obliged to wander in search of truth.

Boyle says, when treating of the "origin of form and qualities," * "There is one universal matter common to all bodies, an extended, divisible, and impenetrable substance."

And in the "Sceptical Chemist." † "But the Aristotelian hypothesis (*i.e.*, of the four elements) is not comparable to the mechanic doctrine of the bulk and figure of the smallest parts of matter, for from these more universal and fruitful principles of the elementary matter, may spring a great variety of textures, upon whose account a multitude of compound bodies might greatly differ from one another." In p. 282:—"Now if it be true, as 'tis probable, that compound bodies differ from one another, in nothing but the various textures, resulting from the magnitude, shape, motion, and arrangement of their small parts, it will not be irrational to conceive that one and the same particle of universal matter, may by various alterations and contextures be brought to deserve the name sometimes of a sulphureous, and sometimes of a terrestrial or aqueous body."

He attacks severely the four elements, the three elements, and the five elements, and justly complains of the "intolerable ambiguity" of the writers on chemistry, and "their playing upon words," as their mode of using salt, sulphur, and mercury decidedly is.

To continue from Boyle. "It seems probable that at the first production of mixt bodies, the universal matter whereof they consist was actually divided into little particles of several

* Vol. I., p. 197. The philosophical works of the Hon. Robert Boyle, Esq. Abridged, methodized, &c., by Peter Shaw, M.D. 2nd edition, 1738.

† Vol. III., p. 266.

sizes and shapes variously moved. * * * * 'Tis also possible, that of these minute particles many of the smallest and contiguous ones were associated into minute masses, and by their coalitions constituted such numerous little primary concretions, as were not easily separable into the particles that compose them." *

† "And indeed if we consider how far the bare change of texture, whether made by art or nature, can go, in producing such new qualities, in the same parcel of matter; and how many inanimate bodies we know to be denominated and distinguished, not so much by any imaginary substantial form, as by the aggregate of these qualities; and that the variation of figure, size, motion, situation, or connection of the corpuscles, whereof any of these bodies is composed may alter the fabric of it; we shall have cause to suspect, that there is no need that nature should always have elements provided, whereof to compose mixed bodies; and that it is not so easy as chymists and others have hitherto imagined, to discern which among the many different substances, without any extraordinary skill, to be obtained from the same portion of matter, ought, exclusive of the rest, to be esteemed its elementary ingredients; much less to determine what primogeneal and simple bodies conspired together to compose it." * * * *

"Our experiment affords us a considerable argument in favour of that part of the mechanical hypothesis which teaches inanimate bodies to differ from one another, but in the magnitude, shape, motion, texture, and, in a word, the mechanical properties of the minute parts they consist of." Page 360. At the end of the chemical doctrine of qualities, p. 441,—"In short, these hypotheses greatly hinder the progress of human knowledge, that introduce morals and politics into philosophy, where all things are transacted according to mechanical laws."

* Vol. III., p. 263. † Page 350.

The doctrine of elementary atoms, either in the sense of indivisible or merely undivided, leads us to suppose all things formed of one substance only; and this seems to have been the notion of those who have held it up as a solution of the question, although generally it has not been carried out far enough to arrive at the difficulties. If all the elementary bodies are the same as this notion supposes, every change is made by change of position of the particles. This is the early doctrine again.

In speaking of the atoms, Boyle says, "little particles of various sizes and shapes variously moved." This "variously moved" introduces a force; it is the introduction of various powers and qualities, and, consequently, of what we term elements. This shews the difficulty of obtaining correct language, when Boyle, whose object was to be accurate, seemed to have overlooked this in those few but important words.

I may quote the graphic words of Boyle, shewing what confused ideas reigned on the nature of combination. "Helmont we know, too, asserts that all mixed bodies spring from one element; and that vegetables, animals, marcasites, stones, metals, &c., are materially but simple water, disguized into these various forms by the plastic virtue of their seeds."* "Aristotle tells us, that if a drop of wine be put into ten thousand measures of water, the wine being overpowered by so vast a quantity of water, will be turned into it. But if this doctrine were true, one might hope, by melting a mass of gold and silver, and by but casting into it lead and antimony, grain after grain, we might, at pleasure, within a reasonable compass of time, turn what quantity we desired of the ignoble into the noble metals."†

Lemery, born about twenty years after Boyle, felt, but not very clearly I imagine, some of these difficulties, as he

* Sceptical Chymist. Vol. III., p. 284. † Page 289.

says, "the first principle that can be admitted for the composition of mixts is an universal spirit, which being diffused through all the world produces different things according to the different matrixes or pores of the earth in which it settles. But, because this principle is a little metaphysical, and falls not under our senses, it will be fit to establish some sensible ones." *

Here, again, is a notion that matter is one, but with an addition that the different properties of its parts are caused by abstract qualities; here there is nothing said either of particles or transformations. It would seem as if matter existed in some condition as one, and received qualities from a spirit which had the power of giving various qualities. We see no variety of agents, but one agent; so that a will and design are constantly required to be present.

This is manifestly a backward step from Boyle, who was not well followed up in his own direction.

Mayow, of Oxford, wrote in 1673 definite opinions as to the combination of acid and alkali, shewing that sulphuric acid separated nitric from saltpetre, and formed vitriolated tartar, or sulphate of potash, that therefore the nitric acid was not destroyed when it united with potash, seeing that it could be separated again by the sulphuric acid. Although we cannot separate the sulphuric acid from the potash, "it is not because these salts have mutually destroyed each other, but because there is nothing in nature which unites with each more firmly than they do among themselves."† He also clearly shews that alkalies throw down metals from their solutions.

"These important observations of Mayow were continued still further by Geoffroy Senior. He considered the order in

* Page 2. Lemery's Course of Chymistry. Translated by Walter Harris, M.D. London, 1686.

† Tractatus quinque Medico Physici, 1674. Chapter, "De salium contrariorum congressu et precipitatione."

which bodies separated each other from a given body as constant. Thus metals are separated from acids by the absorbent earths; the absorbent earths are separated from acids by volatile alkalies, while volatile alkalies are separated by the fixed alkalies. He drew up, in consequence, the following tables, exhibiting the order in which bodies separate each other from a given substance. At the head of each column is written the name of the substance with which the bodies, enumerated in the column, combine. Below it are arranged all the bodies capable of uniting with it. That which separates all others is placed uppermost, and that which is separated by all the others is placed undermost, and the others in the order of their separations. Thus, in the first column, the fixed alkalies separate all the bodies below them from the acids. The volatile acids separate all except the fixed alkalies. The absorbent earths separate the metals, and the metals are separated by all the other bodies in the column." *

ACIDS.	MURIATIC ACID.	NITRIC ACID.	SULPHURIC ACID.	ABSORBENT EARTHS.
fixed alkalies	tin	iron	phlogiston *	sulphuric acid
volatile alkalies	antimony	copper	fixed alkalies	nitric acid
absorbent earth	copper	lead	volatile alkalies	muriatic acid
metals	silver	mercury	absorbent earth	
	mercury	silver	iron	
	gold		copper	
			silver	

FIXED ALKALIES.	VOLATILE ALKALIES.	METALS.	SULPHUR.	MERCURY.
sulphuric acid	sulphuric acid	muriatic acid	fixed alkalies	gold
nitric acid	nitric acid	sulphuric acid	iron	silver
muriatic acid	muriatic acid	nitric acid	copper	lead
acetic acid		acetic acid	lead	copper
sulphur			silver	zinc
			antimony	antimony
			mercury	
			gold	

* Geoffroy's expression is *principe huileux*. Kopp's Geschichte der Chemie.

LEAD.	COPPER.	SILVER.	IRON.	ANTIMONY.	WATER.
silver copper	mercury calamine	lead copper	antimony silver, copper, lead	iron silver, copper, lead	alcohol salt

This, on Geoffroy, is taken from Thomson's Chemistry, edition of 1817. The tables are in all the older chemical books. I have not seen the original.

Although these tables were extended and improved by Gellert and Sage, they were not altered in principle until the middle of last century. Although every thing is indefinite here, that is, unfixed by numbers representing either weight or measure, a certain order is given to the substances known to chemists, a classification is produced, certain laws are laid down, and some relation of one body to another is given, at the same time bodies begin to get a definite character and position, and are not mixed up in a confused mass together, capable at any moment of playing the most fantastic tricks with the operators, and converting the chemist from a naturalist to a magician.

In speaking of atoms we must not confine ourselves to bodies that cannot be divided; Newton and others being satisfied with such as are not usually divided. Newton's well known words are these:—" It seems probable to me that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures, and with such other properties, and in such proportion to space, as most conduced to the end for which He formed them; and that these primitive particles being solid, are incomparably harder than any porous bodies compounded of them; even so very hard as never to wear, or break in pieces; no ordinary power being able to divide what God himself made one in the first creation. While these particles continue entire, they may compose bodies of one and the same nature and texture in all ages; but should they wear away,

or break in pieces, the nature of things depending on them would be changed. Water and earth, composed of old worn particles and fragments of particles, would not be of the same nature and texture now with water and earth composed of entire particles in the beginning. And, therefore, that nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations and motions of these permanent particles; compound bodies being apt to break, not in the midst of solid particles, but where those particles are laid together, and only touch in a few points."*

This is the perfection of the mechanical theory, unaided by chemistry. It provides for a diversity of particles in size, shape, and other properties, as well as diversity of arrangement, and as this explains what may be called the external phenomena of combination, it is, to a certain stage, probably the correct one. Our difficulties arise when we think on the constitution and mode of combination of these particles. On the first Newton scarcely allows any argument, on the second he has not said sufficient to satisfy the demands of chemistry.

Dr. Shaw, although not a great discoverer, was a clear-headed man, and his chemical lectures, in 1731, 1732, and 1733, are amongst the first in which we see the matter plainly and practically handled, following up the instructions of Boyle, which he carefully edited. †

At p. 146, he says:—" We must, therefore, observe that the more intelligent of the modern chemists do not understand by principles, those original particles of matter of which all bodies are, by the mathematical and mechanical philosophers, supposed to exist. Those particles remain undiscernible to

* Newton's Optics, near the end.

† Chemical Lectures, publickly read at London in the years 1731 and 1732, and at Scarborough, in 1733, for the Improvement of Arts, Trades, and Natural Philosophy, by Peter Shaw, M.D., F.R.S. Second edition, 1755.

the sense, though assisted with the most finished instruments, nor have their figures and original differences been determined by a just induction. Leaving, therefore, to other philosophers the sublimer disquisition of primary corpuscles or atoms, of which many bodies or worlds have been formed by the fancy, genuine chemistry contents itself with grosser principles, which are evident to the sense, and known to produce effects in the way of corporeal instruments."

As it is clear that matter has various appearances, so if there be only one matter, it must be capable of putting on various forms. This variation of forms has been given as an innate power of matter, and if we extend this idea, we come to the conclusion, more or less clearly expressed by many writers, from the earliest times, that matter itself has no qualities; but that we perceive only the qualities it has put on. Quality means, then, the true matter, or sensible thing which we see. A great many metaphysical, as well as physical difficulties have been removed by allowing a greater number of elements, leaving the difficulties to be solved of a much more profound character.

The remarkable position given to character, making it play the part of matter, is seen well in Hooke's works.*

"I conceive the whole of realities that in any way affect our senses, to be body and motion. By body I conceive nothing else but a reality that has extension every way, positive and immutable; not as to figure, but as to quality; and that the body, as body, is the same, whatever figure it be of: as a quart of water is a quart of water, or a certain quantity of body, though contained in a globe, cylinder, cone, cube, quart-pot, or any other figured containing vessel; and as body, it is indifferent to receive any figure whatever; nor has it more extension in the one than in the other vessel, nor can it have

* The Posthumous Works of Robert Hooke, M.D., F.R.S. Folio. 1705. Pages 171-2. He died in 1703.

less; nor is it more essentially a body, when solid, as ice, than when fluid; that is, the minims of it are equally disposed to motion or rest in position to each other; and therefore body, as body, may as well be, or be supposed to be, indefinitely fluid as definitely solid; and, consequently, there is no necessity to suppose atoms, or any determinate part of body perfectly solid, or such whose parts are incapable of changing position one to another; since, as I conceive, the essence of body is only determinate extension, or a power of being unalterable of such a quantity, and not a power of being and continuing of a determinate quantity and a determinate figure, which the anatomists (or atomists) suppose. These, I conceive, the two powers or principles of the world, to wit, *body* and *motion*, uniformity of motion making a solid, and difformity of the motion of the parts making a fluid, as I shall prove more at large by and by."

* * * * "As for *matter*, that I conceive in its essence to be immutable, and its essence being expatiation determinate, it cannot be altered in its quantity, either by condensation or rarefaction; that is, there cannot be more or less of that power or reality, whatever it be, within the same expatiation or content; but every equal expatiation contains, is filled, or is an equal quantity of *materia;* and the densest or heaviest, or most powerful body in the world contains no more materia than that which we conceive to be the rarest, thinnest, lightest, or least powerful body of all; as gold for instance, and *æther*, or the substance that fills the cavity of an exhausted vessel, or cavity of the glass of a barometer above the quicksilver. Nay, as I shall afterwards prove, this cavity is more full, or a more dense body of æther, in the common sense or acceptation of the word, than the gold is of gold, bulk for bulk; and that because the one, viz., the mass of æther, is all æther; but the mass of gold, which we conceive, is not all gold, but there is an intermixture, and that vastly more than is commonly supposed, of æther with it; so that

vacuity, as it is commonly thought, or erroneously supposed, is a more dense body than the gold as gold. But if we consider the whole content of the one with that of the other, within the same or equal quantity of expatiation, then are they both equally containing the *materia* or body."

This argument will not appear so conclusive to his readers, but it serves well to shew how unsettled were the opinions on matter, when a man of Hooke's high standing spoke in this manner. He could not believe in epicurean atoms as he called them, and he had as little faith in "the four elements, the three chemical principles, magnetism, sympathy, fermentation, alkaly, and acid, and divers other chimeras." The quotation is by no means intended as a specimen of Hooke as a philosopher. Aristotle's aether and the abstract ideas of Plato are still perceptible here in confusing the reason and obliterating the observation.

When a definite form is given to the minutest particles of bodies, it leads to their indivisibility by a very easy reasoning, but the moment these ideas become vague, or if a prime matter is allowed, taking various forms, or a few elements changing into each other, essentially the same things, then infinite divisibility is more likely to lay hold of the imagination.

Des Cartes, speaking of the theory of Democritus, says, "it is rejected because he supposed the existence of indivisible corpuscles, because there was a vacuum around each, which could be demonstrated impossible, and because there was weight given to each, whereas no body has it when taken alone, its existence depends on the situation and motion of other bodies."* But he thinks also that natural things might have been made in various ways. We must explain the phenomena as we best can. "Ita non dubium est, quin summus rerum opifex, omnia illa quae videmus, pluribus diversis modis potuerit efficere."† He also retained the four elements.

* Renati Des Cartes, Principia Philosophiae. Page 219. Amsterdam, 1677.
† Prop. ccii.

We find Sennertus, about the same time, expounding Aristotle's opinions on the elements, and looking to the authority of character more than experiment, but he does not give a fifth element such as Aristotle's ether. The argument he gives that there are only four is curious, and another of the interminable varieties of method in which vagueness thinks. "There are two right motions, one from the mean, the other to the mean, there will, therefore, be an equal quantity of simple bodies, subject to these two simple motions, one which absolutely is heavy, called earth; another absolutely is light, called fire. But because nature wishes the world to be one, but contrary extremes cannot constitute one, she always couples the extremes by means (per media), and connects the last of the superior kind with the first of an inferior. This mean is therefore required. But this cannot be *one*. Because, if so, it would occupy the mean place between the extremes, or between the centre and circumference, and so no right motion (rectus motus) could be given to it. For it could neither be moved to the middle, nor from the middle, nor could it be called more or less heavy. It is necessary, therefore, to have two means, one light, and which may be moved from the middle upward, in respect of which it is heavy, and is called air; the other heavy, and tending to the middle, in respect of which it is light, which is called water. There are, therefore, four elements, fire, air, water, earth. There cannot be a fifth for the same cause, that there cannot be only one mean. But if any one will desire to establish five, his senses and experience disprove it." *

He endeavours to establish a difference between the pure fire and the common drudge of life, and quotes Scaliger to support him, but in neither of them do we find any distinct ideas of what these differences consist. We are confounded by the great part which hot, cold, moist, and dry, have to play, and it

* Danielis Sennerti *Vratislaviensis* Epitome Naturalis Scientiae. Oxoniae, 1632. Page 185.

never occurs to them that hot air is air and heat, and that moist air is air and moisture, but they are qualities put on by air at will, or at command, by accident, by growth, by motion, or by life. The qualities of the elements are not substantial forms, but accidents; the original first matter forms the elements which put on various forms, and so perform everything that elements have the power of accomplishing. The changes are performed by generation, fermentation, and corruption.

Baumé, a believer in the four elements and phlogiston, in 1773, says, that it is of no advantage to consult the ancient chemists, they are so ambiguous; and says: *—

"Is it not probable that nature combines the elements in a direct manner by twos and twos, and by threes and threes, by means absolutely unknown to us? If these simple combinations exist, they will be secondary principles, or principles made from principles (principes principiés), of which nature makes use to form compound bodies. Knowledge, on this subject, entirely fails. We have no information on the immediate combinations of the four elements; we only know that they have such a disposition to mix, that it is impossible to have them perfectly pure and isolated from each other."

We have here an opinion freed from the shackles of salt and sulphur, but not beyond the early times, and I think not so penetrating, although as practical as Roger Bacon's.

It will be interesting here to quote Bishop Watson, a man who brought into chemistry more common sense and less pretension than many less penetrating observers. Speaking of elements, he says, "by chemical elements, which are the last products of chemical analysis, we are to understand, not very minute indivisible particles of matter, but the simple homegeneal parts of bodies which are not capable, as far as our experience teaches us, of any farther resolution or division, except in a mechanical sense, into

* Page 119, Vol. I., of "Chymie Expérimentale et Raisonnée."

similar parts, less and less, without end, as water into vapour, more or less, subtile and attenuated. Aristotle and his followers esteemed earth, air, fire, and water, to be elements, simple and uniform in their several kinds, essentially distinct and utterly incapable of being converted into one another, yet easily uniting together, and by their different arrangements, and properties and mixtures, composing every body in the universe. Many modern chemists have adopted this idea; others have increased the number of elements, by adding a saline principle; others have contended that some of these elements, air and fire for instance, are themselves compound bodies; and others, lastly, are persuaded, that there is only one elementary homogeneal principle, and that all the varieties of bodies, as well as of what are most commonly esteemed elements, ought to be attributed to the different magnitudes and figures of the particles composing them; and as the component parts of water or air, or any other body, are by no means supposed to be elementary particles of matter, but to be made up of different numbers of elementary particles, arranged in different forms, it may be thought probable, that mechanical causes may either diminish or augment the number, or change the disposition of the particles, and thus effect the several varieties observable in nature.

"It would be improper in this place to enlarge on a subject, concerning which both ancient and modern philosophers have been so much divided in opinion. Their great diversity of sentiment may suggest a suspicion that the full comprehension of it does not fall within the reach of the human understanding. The following observation may, perhaps, tend a little to illustrate this matter. Let us suppose that this terraqueous globe was not surrounded with any air or atmosphere, and that by an approach to the sun, or an increase of subterranean fires, by some means or other it should become exposed to a heat four times greater than the medium heat of summer, which we may reckon to be about 60 degrees

of Fahrenheit's thermometer; then would an atmosphere be quickly formed around it, all the water on its surface, most of the juices of plants and animals, and a great variety of mineral particles, would be raised up in vapours and exhalations, and whilst the heat continued would be kept suspended in an elastic state, and constitute an atmosphere analogous, as it may reasonably be imagined, to the chaotic state of our present atmosphere, only differing from it in this, that it would require a greater degree of heat in order to keep the particles of matter from coalescing into one heterogeneous mass. Again, in the present state of the atmosphere, suppose that a great degree of cold should continue unabated for any length of time, all the water on the surface of the earth would be changed into a solid transparent stone, which might be dug out of its quarry, and then employed in building as well as marble, or any other species of stone; all the particles of air would be bronght closer together; some of them which were the least elastic would be reunited: and imagining the cold to be indefinitely increased, what reason can there be against supposing that the whole atmosphere would be reduced into a solid state, forming a heterogeneous crust on the surface of the earth; the thickness of this crust, supposing it to be as dense as marble, would be about four yards? It will easily be understood, that water, and air, and earth are, upon this hypothesis, but variations of the same element introduced by heat.

"That the atmosphere which surrounds the earth was originally formed from the chaotic mass, by having the more subtile parts of which that mass consisted, elevated and put into an elastic state by means of heat, seems not altogether improbable. We find the atmosphere or firmament immediately succeeding the formation of light; now, if the effect of that light was heat, be the form or matter of it what you please, then would such particles of the shapeless jumble as were capable of being evaporated with that degree of heat,

be elevated in an elastic state, and a division or separation would be made in the midst of the great abyss, between the waters which were of a nature subtle enough to be converted by that degree of heat into an elastic fluid, constituting the firmament or atmosphere, and the waters which could not be evaporated in that degree of heat, but still remained covering the surface of the globe, being not collected into one place, that the dry land might appear, till the third day. This notion of the atmosphere and its formation, seems to be conformable enough to Newton's opinion expressed in his letter to Mr. Boyle. "I conceive the confused mass of vapours, air, and exhalations, which we call the atmosphere, to be nothing else but the particles of all sorts of bodies of which the earth consists, separated from one another and kept at a distance by the said principle," a principle of repulsion." *

Here we have the one element again or matter treated simply as matter generally, in the same way in which metaphysicians have dealt with it, leaving heat and, probably as by others expressed, some general plastic power to form all the modifications. This is much too indefinite for science, and it is surprising that it should have been deemed sufficient even in Bishop Watson's time. There is no attempt to examine the degree of heat which may convert a solid body into a permanent gaseous one, and no difficulty perceived in the persistence of the atmosphere in a gaseous state whilst exposed to no more cold than the solid or liquid substances around us, or the persistence of the water in a liquid state, or the permanency of the earth, why it did not occasionally send off rocks into vapour or rarefy them into air; nor is there any attempt to find what strange powers may cause the diversity of structure. Although not expressed, these words were evidently

* Chemical Essays. By R. Watson, D.D., F.R.S., &c. Vol. I., p. 100. 4th Edition. London, 1787.

written under the idea that the powers of nature worked rather arbitrarily and capriciously; how, is not distinctly seen. When the mixture of pure elements is spoken of as forming all bodies, there is a reason given clear enough for the changes, and the chief blame attached to those who believed in this is, that they did not obtain the amount of each needed to form any one distinct substance. This proves, as it appears to me, that they held their opinions by a very slender tie.

Boscovich, in his theory of natural philosophy, in 1759, gave the fullest scientific expression of the unsubstantial theory of matter, which may be called the *dynamical*. The book is not common, and not to be found in this neighbourhood. I shall take Dr. Daubeny's description: "He supposes that matter is made up of a number of unextended indivisible points, which, however, never touch each other, owing to the mutual repulsion subsisting between them, so soon as they come within a certain distance of each other; which repulsion increasing gradually in proportion as they are made to approach nearer and nearer, becomes at length too powerful for any force to overcome." *

In this theory we have again revived in another form the idea that all matter comes from a non-material, or what we may call a spiritual force, nor is it easy for us to conceive how it could have any other origin. It is a revival of the doctrine of the mind and of the soul being the origin of matter, or of the early opinion that numbers were the true beginning, or in other words, abstract forces. But we have to do with matter when it is formed, not with its production; and when these unextended points have obtained existence, we are obliged to reason on them as if real. One result, however, affects our subject, that by this theory, matter may cease to be infinitely

* An Introduction to the Atomic Theory. By Charles Daubeny, M.D., F.R.S., &c. Second edition, Oxford, 1850. p. 84.

divisible. It does not of necessity cease to be so, as we may readily believe, in the constancy of these points; but we may also imagine the power dispersing in various directions, and the absolute amount of force existing in one particle to lie diffused throughout the whole world, or any given amount of space.

When the particles are formed, Boscovich deals with them exactly as with matter, formed of indestructible atoms. "Thus he supposes that the points of matter alternately attract and repel each other, according to the distance that separates them, until they either come very close to, or are removed to a comparatively great distance from each other; in the former case they are repelled, in the latter attracted; the former force preventing mutual contact, the latter, which, when considered as acting between the earth and bodies upon it, is no other than gravitation, drawing them all together."

The *Encyclopedie Methodique*,* in an article written by G. Morveau, 1786, may be presumed to give the advanced opinion of chemists. He says,

"Are there really different degrees of saturation of the same salt? or is not the union which it contracts with that portion which exceeds the point of saturation, the effect of super-composition, as in combination with a third foreign body? This is a question which merits all our attention, not only because it is of interest in the general theory of chemical attraction, but rather because it is of importance that we should understand what is the character of the affinity before we attempt to submit it to calculation, or even deduce a satisfactory explanation of the phenomena which depend on it. I confess that the idea of divers degrees of saturation of one body with another appears to me repugnant to all the notions

* Vol. I., p. 560-1. Encyclopedie Methodique. Chymie Pharmacie et Metallurgie. Paris, 1786—1815.

we have hitherto acquired of the method by which combinations are formed. I can readily conceive that the point at which water is saturated with a salt may change according to the temperature, and that cold water may absorb more of the gaseous acid; the increase or diminution of the matter of heat in the solvent changing the respective disposition, the density, and perhaps the figure of the molecules, it is not astonishing that the attractive force should be modified by these changes; that they should result in a contact more or less perfect, and that the power of affinity should be able by this means to protect from *(the influence of)* the law of gravitation a greater quantity of matter, in the one case, than in the other; but we have nothing of this kind in the hypothesis under consideration; the circumstances are the same; the point of saturation cannot change, because it is the effect of a cause which does not change.

"To make this clear, let us ask what is saturation? Every chemist will say that he understands by it the condition in which a compound is, when neither of its constituents can receive or retain in combination a greater quantity of the other. Such is the rational and necessary acceptation of the word saturation, otherwise it becomes void of sense; then to suppose, preserving this acceptation, that a substance may be saturated with different quantities of the same substance, is really to affirm two contraries."

"* * * * Now, when a new quantity of any of their principles is added to one of these perfectly neutral salts, there is nothing to prevent an attraction between them, and even in a degree capable of producing solution, combination, or affinity; but it must be remarked that this affinity is not that of an acid to a base, or a base to an acid, but of the neutral compound, with the portion that was added; whence it follows; 1st, that it has no effect on the previous composition which remains in its integrity, as if the neutral salt were super-

compounded with a foreign body; 2nd, that the point of saturation is not changed; and 3rd, that the power which unites this portion which is added to the neutral salt, may be much weaker than that which unites the same substance to the same, at the point of saturation, without causing a contradiction."

Page 566. "I shall terminate this section by a short résumé of the principal characters, which are capable of forming a methodic division of affinities, and which may have been lost sight of in the course of the preceding discussions.

"1st. *Two bodies of the same nature*, whether simple or compound, may unite and form a third, as homogeneous as either of the two before the union. This is called *affinity of aggregation*.

"2nd. *Two bodies of a different nature*, simple or compound, may unite without undergoing any change in their first composition, if they are compound. This is called *affinity of composition*.

"Two out of three bodies may shew a preference and combine, leaving the third at liberty; these bodies may either be simple or compound, provided their composition does not change, and they are found in a condition favourable to contact. This is still affinity of composition; whether the three bodies have been put separately into the mixture, or two been previously united, whilst the superior affinity of the third has destroyed their union, by what is called precipitation.

"*Three or more bodies*, exposed to contact, may unite in such a manner as to form only one homogeneous mass. This is nevertheless affinity of composition, although two only unite at first, and a third is added to the first two, and so on successively.

"3rd. *Two bodies* incapable of combining, become so when one or the other has either been decomposed or supercomposed.

In this case, the affinity of composition, which produces the union, takes the name of *disposing affinity*.

"4th. *Two or more compounds* being placed in circumstances, suitable for bringing into play the respective affinities of their component parts; either there is a change and new products formed without our being able to determine which is the most powerful affinity under which they act, or the first composition remains, contrary to the order indicated by the superior affinity of the principle of one of the component parts to the affinity of the other. In these two cases we say that they are not the relations of affinity of one body to another, *but affinities of concourse*, otherwise called *double affinities;* in a word, the sum of all the united affinities which are needed to explain these phenomena.

"Two bodies being put in contact, the compound which results is supercompounded, or united with an excess of one of the principles. This tendency to supercomposition, is sometimes so strong, that when the least affinity of a third body interferes with it, the proportions of the first compound are changed, and the neutral state destroyed. This tendency may cause the production of crystals, with excess of base, in an acid liquor. To distinguish this force, we shall call it the affinity of a compound for an excess of one of its constituents, or for shortness, *affinity of excess*, which will be enough to recall the idea when it has been well grasped. However paradoxical some of these propositions may appear, I have no fear of their being called in question after the proofs I have given; and if they are well founded, it will readily be granted that they ought to form one of the most important elements in the calculation of affinities."

Here now we have what Morveau has given, as the most certain of the laws of affinities known among chemists at his time. He adds, however, that they certainly scarcely deserve the title. The following are additions:—

* "1st. There is no chemical union, if one of the bodies be not sufficiently fluid to allow the molecules to obey the law of affinity, which carries them from (mere) proximity to (actual) contact.

"2nd. Affinity takes place only among the smallest integral molecules of a body.

"3rd. From the affinity of one substance with another, we cannot know the affinity of a compound; we cannot know the affinity of a compound of one of these substances, with one or the other in excess.

"4th. The affinity of composition has no efficacy, unless it can overcome the affinity of cohesion.

"5th. Two or more bodies, which unite by the affinity of composition, form a substance which has new properties distinct from those which belong to each of the bodies before combination.

"6th. To give effect to affinities, a particular temperature is necessary, which renders the action either slow or rapid, invalid or efficacious."

These feeble and incipient attempts at laws may surely surprise us, written as they are almost within the memory of some of the living. There was at this time an attempt to measure the force of affinities as the only method of obtaining results, and there was not yet seen the absolute necessity of having bodies kept uniform by a constant and absolutely similar composition, much less to bring this absoluteness of composition under the forms of natural law, and we might add also, logical necessity.

It should be noticed that the remarks about saturation shew that compound proportion was not in the least degree understood in the present sense. Its possibility is even denied.

The aim was to find the strength of affinity. When it is

* Remarks in italics on the various laws, from p. 567 onwards.

said that we cannot find the affinity of a compound from knowing that of its parts, it refers to affinity dynamically; no hint is given as to the knowledge of this amount quantitatively. Proportion generally may be said to be excluded.

Although these opinions give a pretty fair idea of the state of the chemical mind at the several dates, we must return to an earlier date again to follow up another direction.

CHAPTER VII.

PHLOGISTON PERIOD AND PROGRESS OF THE BALANCE.

In the 17th century innovations were beginning in chemistry, as we have already seen, but as usual, these did not all take one direction. Van Helmont put his little *archaeus*, a kind of intelligent agent, but with less independence than that of Paracelsus, into the stomach, to do the work which he could find no way of accomplishing by merely physical means. Thus things began a new mystical direction. Becher, in the *Physicae Subterraneae*, ridicules his archaeus and chimeras, and the whole host of "impudent chemists" also, who assert that they obtain salt, sulphur, and mercury, from all bodies, even animals and vegetables. He does not hesitate to call these the greatest falsehoods. He calls "elements the genuine and true things of which bodies consist, and from which others are made and prepared."* But as he held on by the four elements, we are not able to find in him much material.

He had the merit of raising inquiry in a high degree, and of bringing forward his great admirer, Stahl, who introduced phlogiston. In this chapter we have a class of men who have made another advance in experimenting, and whose works are the first which living chemists can, without difficulty, peruse. The advent of oxygen into science was preceded by a century of vague prophesyings. The use of the balance was becoming general, but men had no idea of the accuracy with which nature weighed, although they had long used the proper principle of making the earth the arbiter, by trying which side of the scale she drew most willingly towards her. They

* Phys. Sub. Lib. i., sect. iii., cap. i., No. 12.

had no idea of the fineness of her touch, and her absolute refusal to make any allowance for inaccuracy in the construction of instruments. It was not even known that all bodies could be compared by their weights; why should they not as well be known by their lightness? This plan had its fair trial. By a curious circle of reasoning, it was decided, that what we call oxygen, which makes an oxide, or calx of a metal, was sulphur; afterwards it was the principle of combustion; not such an erroneous idea. Now oxides or earths were, *of course*, simple bodies; when they were reduced to metals in the fire, they combined with phlogiston; they became lighter. Therefore phlogiston had the principle of lightness in it. The rule generally is, that we should begin wrong. We now say the metal is simple, and by uniting with oxygen, it becomes a compound, and is heavier. As the metal burns and gives out heat, they said it gives out its phlogiston, and loses its principle of lightness.* Stahl calls it sulphur. This would scarcely come under our view had it not been the cause of so many inquiries in the same direction, as to bring about a result, derived from an analysis of all the oxides, and a careful comparison of the weight of the metals, with the weight of the oxides, whether produced by combustion or oxidation in the fire, or precipitated from their acid solutions. Even this strange theory tended in the right direction, although at first threatening to take a mystical course. We could scarcely have anticipated this difficulty of proving that all bodies have weight and not lightness, but our forefathers encountered it, and it may yet come to the struggle again, renewed in a higher form, when we have to deal with those physical existences, now called *imponderables*.

I am not aware that any one went into the subject with care before Bergman. He may be said to have introduced modern analysis. Before him analyses were not superior to

* p. 277. "Traité de Soufre" Traduit de l'Allemand de Stahl. Paris, 1767.

those speculations about the constitution of bodies which in former chapters have been passed over. I may indeed cite here Roger Bacon's syntheses of bodies from the four elements, as the earliest examples of an endeavour to shew how so many bodies can be formed from few elements, and on the other side, as the fullest example I know of early analysis, and perhaps the very first in which numbers are used in connection with elements. They are intellectual strivings after quantitative analysis.

"There is, therefore, one different kind where fire and air are greater; 2ndly, where fire and air are less; 3rd, where fire and water are greater; 4th, where fire and water are less; 5th, where fire and earth are greater; 6th, where fire and earth are less; 7th, where air and water are greater; 8th, where air and water are less; 9th, where air and earth are greater; 10th, where air and earth are less; 11th, where water and earth are greater; 12th, where water and earth are less; and so you have two diversities. Next you have three diversities; 1st, where fire, air, and water are greater; 2nd, where fire, air, and water are less; 3rd, where air, water, and earth are greater; 4th, where fire, water, and earth are greater; 5th, where air, water, and earth are less; 6th, where fire, water, and earth are less; and in this manner, if you divide those methods, you obtain from the first 16, from the second 64, from the third 47, from the fourth 18, in all 145. I will now speak of the fourth diversity, fire much, air less, water much, earth less; second, air much, water less, earth less, fire less, and so being ingenious, you may draw out all these diversities to light."

A manuscript copy of Dr. Cullen's lectures in 1762-3 in the laboratory of Owens College, Manchester, from the late Dr. Henry's library, mentions four elements, which, by simple combination, could be formed into seven, but any proportionate combination to account for the number in nature, is not given.

These lectures shew him to have been an exceedingly clear and rational expounder of science. With good common sense he waits for more knowledge when science fails, fully shewing why he became famous, although he published very little. Reasoning on the state of things at the time, he says, "It appears, then, that we know of no physical element, nor any chemical principle, nor are we acquainted with any body which has fixed and permanent qualities."

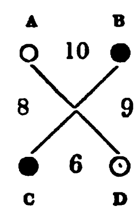
He afterwards adds, "Having laid down and demonstrated this fundamental proposition, viz., that the changes of the qualities of bodies are all of them produced by combination or separation, I now proceed to inform you that combination depends upon *attraction*, that is, the attraction of cohesion, whereby the small particles of bodies very near each other are disposed to approach, and in a certain contiguity to remain coherent together."

He then goes on to explain simple elective attraction and double elective attraction by diagrams, like those below, where the lines ought to be drawn straight from C to B, and from A to D. This appears to be earlier than Bergman, who at that time had published nothing on chemistry. I can find no internal evidence of their being written later than they profess to be, the binding itself being old.

Dr. Cullen was professor of chemistry at Glasgow, and Dr. Black attended his lectures, before being appointed his successor, on the removal of Dr. Cullen to Edinburgh, in 1756. In the Annals of Philosophy, Vol. III., p. 554, Dr. Thomson says:—"My knowledge of Dr. Cullen's opinions was derived from the late Professor Robison, of Edinburgh, who had the means of information, and, as he was a particular friend and great admirer of Black, is entitled to credit. Now, he informed me that Dr. Black's explanation of double decompositions, which he annually gave in his class, had been originally broached by Dr. Cullen. This was the circum-

stance that induced me to broach Dr. Cullen's name along with Black and Bergman.

"* * * As to Dr. Black, I consider myself as acquainted with his opinions, because I attended his lectures; and there are thousands in Great Britain who did the same, and who cannot but recollect the facts that I shall state. Dr. Black taught that bodies combine in definite proportions, and he explained double decomposition by means of diagrams, not, indeed, the same as those of Mr. Higgins, but much simpler and more elegant. I have been informed by Prof. Robison, that he employed these diagrams from the very beginning of his career, as a professor. One of them is given in page 554, Vol. I., of his printed lectures. I have no doubt that all similar diagrams, published in London, by Fordyce, &c., were derived from the same source. Now, could the doctrine of definite proportions be taught, and could double decomposition be explained in this way (I quote Dr. Black's explanation), let the bodies A and B be united with a force, 10; and the bodies C and D with a force, 6. Suppose the attraction of A for C to be 8, and that of B for D to be 9, if we mix these bodies, A will unite with C, and B with D. To me they conveyed just as much of the atomic theory as the perusal of Mr. Higgin's book did."

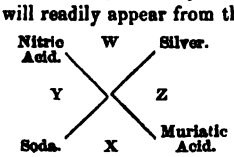


Dr. Robison edited the lectures of Black in 1803, and in a note gives the above diagram and some judicious remarks, shewing, at the same time, that although definite proportion was taken for granted, no general law to account for it had been given.

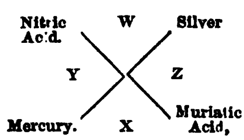
But the question cannot be as to whether Dr. Cullen discovered the atomic theory, (indeed, this extract might have been brought on somewhat later), but whether Dr. Cullen had so far advanced our knowledge of matter as to be the first who gave out the ideas of single and double elective attractions, such as have been attributed to Bergman.

A note gives a fuller account of Dr. Cullen's views; it was written in the year 1759. Elective attractions were in reality definitely laid down and presupposed in Geoffroy's tables; but the investigation and elaboration was needed.* At present we must consider Dr. Cullen as the first who used the words and explanations in the manner afterwards made so famous by Bergman.

* Note E., p. 45., Cullen's Life, by Thomson, p. 570. Appendix.—The following passages from a letter, written by Dr. Cullen to his friend and former pupil, Dr. George Fordyce, of London, in October, 1759, contains his own statement of his views with regard to double elective attractions. "I must give you the manner of considering the subject, which I fell upon last session, and shall continue to employ as the most easy and simple. I begin with your third and fourth cases, and to these one general rule applies, viz., that when two mixts (compounds) are applied to each other, if in each mixt there is a substance, that from the table of elective attractions, is by itself capable of decomposing the other mixt, the attractions between these substances and the substances they attract in the opposite mixt, must always be greater than the attractions subsisting in the mixts applied to each other; and therefore, &c. Thus, if *nitrum argenti* and common salt are applied to each other, as by the table of elective attractions, the nitric acid in nitrum argenti, is by itself capable of decomposing the other mixt, common salt; and the muriatic acid in common salt is capable of decomposing *nitrum argenti*: the attractions between the nitric acid and the soda, with the attraction of the muriatic acid and the silver must be always greater than the attractions subsisting in the mixts, nitrum argenti and common salt, that were applied to each other. This I illustrate by the diagram adjoined. Let there be two rods intersecting one another, and moveable on a common axis at the point of intersection. At the extremities of each let there be placed substances that have an attraction for each of the substances on the extremities contiguous to them, and let the attractions be expressed by the letters W, X, Y, Z. The rest of the illustration will readily appear from the diagrams.



$Y > X$ and $Z > W$ by table.
Ergo $Y + Z > X + W$



$Y > X$ and $Z > W$ by table.
Ergo $Y + Z > X + W$

You see that the prevailing attractions are here determined from the table of single elective attractions.

We are now come to the only difficulty in the affair of double elective attractions in instance past. To this our general rule does not apply.

There is no doubt that, however these opinions might be at the time floating amongst chemists, the works of Bergman were both the fullest and the most important on this subject. From them I shall give rather long quotations. *

" On the different amount of phlogiston in metals,† he says; calces (oxides) do not displace each other, as experience shows, at least, not in the same order as the metals do. May not therefore the quantity of reducing phlogiston in any metal be determined by a comparison of the weights of the precipitated and the precipitating metal? The following experiments will show the answer, but let us first examine, in a general way, those cases which may possibly occur:—

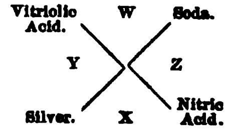
" Let *A* be the precipitating metal, *m* the weight of acid necessary for dissolving 100 of *A*, *x* the quantity of reducing phlogiston in 100 of *A*; *B* the metal to be precipitated, *nm* the weight of the solvent mentioned necessary for dissolving 100 *B*, and *y* the amount of reducing phlogiston in 100 *B*. *n* may be equal to unity, or it may be more or less than unity."

" Let, I., $n = 1$ then $m = nm$."

(In other words, if $n = 1$ the quantity of acid necessary for dissolving the precipitating metal, it will be equal to the quantity necessary for dissolving the precipitated metal.)

" In this case, if $x = y$ there is no difficulty, because the solvent of each dissolves an equal weight, and *B* is able to take from *A* as much of the inflammable material as is necessary for its reduction.

See how it comes out when my new scheme is applied to it. Y and Z are by the table of elective attractions each of them less than W, but greater than X. If, therefore, Y and Z are exactly as much greater than X, as they are less than W, the four attractions would be exactly balanced; but if Y and Z exceed in any degree more than they fall short of W, than Y + Z must be greater than W + X.



* Torberni Bergman, Opuscula Physica et Chemica, pleraque ante seorsim edita, jam ab auctore collecta, revisa et aucta. Holmiae, Upsaliae Aboae, &c. Vol. I., 1779, II., 1780, III., 1786, IV., 1787, V., 1788, VI., 1790.

† Vol. III., p. 136.

" If x is greater than y there appears still no obstacle to prevent complete precipitation.

" But if x is less than y, so that only a part of B can be displaced, a portion of the dissolved precipitant must be sensibly thrown down, so as to act anew, or some other assistance must be given.

" II. Let $n > 1$ et $m < nm$

" With respect to phlogiston, this is the same result as in case I., but the obstacles are less."

(That is, if the acid for dissolving the precipitating metal is less than the acid which dissolves the metal to be precipitated, as in this case, the precipitating metal would not cease its action for want of acid.)

" III.) Let $n < 1$ then is $m > nm$. In this case B cannot be entirely thrown down, unless $nx = y$ or $nx > y$, because only n 100 of the precipitant A is dissolved."

(That is, if it requires more acid to dissolve the precipitating metal than the one in solution, then the metal in solution cannot be quite thrown down, unless it should be found that the amount of phlogiston in the precipitant is equal to the amount in the precipitate, or greater than it.)

Then, after recounting experiments, the first of which are made with a nitric acid solution, he says, p. 139;

" Therefore 135 parts of mercury have reduced completely into the metallic form by means of their phlogiston, 100 parts of silver which had been dissolved and calcined. This had united with four times its weight of mercury, and crystallized in an arborescent form.

" The amount of lead necessary for precipitating 100 lbs. of dissolved silver, amounts to 234 lbs. * * * * * * *

" C. 375 lbs. of shining plates of copper were put into a solution of silver, and were soon covered with a crystalline silver coating. When all the silver had fallen, the copper plates, when well cleaned, were found to have lost 31 lbs. The precipitated silver was found to amount to a cwt. (100 lbs.)

"In order to examine into the power of different solvents, we precipitated with copper a hundredweight of silver, which was dissolved in vitriolic acid, but there were only 30 lbs. of copper used. This, then, enables us, to some extent, to measure the great avidity with which nitric acid seizes on phlogiston, so much excelling the vitriolic acid."

The amount of each metal needed to precipitate 100 lbs. of silver, is given with the experiments, but to save room, I add a list.

"185	lbs.	Mercury dissolve	100 lbs. of Silver.
234	„	Lead	ditto.
31	„	Copper (with nitric acid)	ditto.
30	„	——— (with vitriolic acid)	ditto.
29	„	Iron (with vitriolic acid)	ditto.
88?	„	Tin	ditto.
		Bismuth could scarcely be determined.	
64	„	Nickel	ditto.
92	„	Arsenic	ditto.
37	„	Cobalt	ditto.
55	„	Zinc	ditto.
83	„	Antimony	ditto.
51	„	Manganese	ditto.

Amounts of zinc used to precipitate 100 lbs. of metals.

217	lbs.	Zinc precipitated	100 lbs. of pure	Gold.
416	„	„	ditto	Platina.
44	„	„	ditto	Mercury.
26	„	„	ditto	Lead.
164	„	„	ditto	Copper.
68	„	„	ditto	Tin.
49	„	„	ditto	Bismuth.
70	„	(the solution was difficult)	ditto	Antimony.

Scarcely any precipitation appears with Iron."

Then, at p. 150, there are certain corollaries, of which the following sentences suit best the subject in hand:—

COROLLARIES.

"A. That dephlogisticated metals unite with different acids in a variable manner (*i. e.*, that different amounts of metal unite to different acids). Thus, 100 parts of silver, dissolved in

nitric acid, are reduced by 31 of copper, but if united to vitriolic acid, they want only 30 of copper. In the same way 100 parts of copper, in a vitriolic solution, are restored *to a metallic form* by 146 pounds of zinc, but in a nitric acid solution, 164 lbs. of zinc are wanted. Therefore nitric acid dephlogisticates the metals most, vitriolic acid less, and muriatic acid still less.

" B. Since we added the solutions in a saturated state, it is clear that the mutual quantities of phlogiston in the precipitate and the precipitant are in inverse proportion to the weights. Let the quantity of the phlogiston in a hundredweight of silver be 100, and the amount in a hundredweight of mercury will be 74, in lead 43, in copper 323, in iron 342, in tin 114, in bismuth 57, in nickel 156, in arsenic 109, in cobalt 270, in zinc 182, in antimony 120, in manganese 196. * * * *

" D. Let us see then how those principles before-mentioned may be applied. With respect to silver, which answers to *B*, there is no precipitant or *A*, which acts so as to make $n = 1$. If mercury or lead is used, then $n > 1$, but if copper, iron, tin, bismuth, nickel, arsenic, cobalt, zinc, antimony, or manganese is used, the case is $n < 1$. In the zinc precipitates $n = 1$ is also wanting. Gold, platinum, iron, and antimony, make $n > 1$, all the rest $n < 1$.

" Page 155. According to the experiments produced, the metals richest in phlogiston, are platina, then gold, iron, copper, cobalt, manganese, zinc, nickel, antimony, tin, arsenic, silver, mercury, bismuth, and lead, so that, in some order, it approaches nearer to the first metal. The relative numbers designating the amount found in each, are to be sought by other methods. A trial of each of the metals, so as to obtain the attractions sought for, would be a great labour, if done with sufficient care and sufficiently repeated, but if the work were divided it would be easier. If one would choose for examination mercury, another lead, a third copper, and so on, so as to see their relation with respect to the others, then we

should have a series of experiments, which, if rightly compared, would not only disclose the various properties of each, worthy of observing, but would determine also the relative quantities. In this way, if the absolute value of only one were diligently sought out, all the rest would follow.

Vol. ii., p. 373. "The calces of metals have not that amount of phlogiston which is necessary to the metallic condition, but they are still found not entirely deprived of it.

"Metallic precipitates, when properly examined, reveal to us various mysteries."*

"In the following table 100 parts of reguline metal are in all cases understood to be dissolved:— †

100 parts	of Gold	with the	aerated mineral alkali gave	106	of dry precipitate.
"		"	caustic	110	"
"		"	martial vitriol	100	"
"	Platina	"	aerated mineral alkali	34	"
"		"	caustic	36	"
"	Silver	"	aerated mineral alkali	129	"
"		"	caustic	112	"
"		"	phlogist. (pruss. of pot.)	145	"
"		"	saline	133	"
"		"	vitriolated	134	"
"	Mercury	"	aerated mineral alkali	110	"
"		"	caustic	104	"
"		"	vitriolated	119	"
"	Lead	"	aerated mineral alkali	132	"
"		"	caustic	116	"
"		"	vitriolated	143	"
"	Copper	"	aerated mineral alkali	194	"
"		"	caustic	158	"
"		"	phlogisticated	530	"
"	Iron	"	aerated mineral alkali	225	"
"		"	caustic	170	"
"		"	phlogisticated	590	"
"	Tin	"	aerated mineral alkali	131	"
"		"	caustic	130	"
"		"	phlogisticated	250	"

* Page 390. † Vol. II., p. 390.

100 parts of Bismuth	with the	aerated mineral alkali gave	130	of dry precipitate.
,,	,,	caustic	125	,,
,,	,,	phlogisticated	180	,,
,,	,,	pure water	113	,,
,, Nickel	,,	aerated mineral alkali	135	,,
,,	,,	caustic	128	,,
,,	,,	phlogisticated	250	,,
,, Arsenic	,,	phlogisticated	180	,,
,, Cobalt	,,	aerated mineral alkali	160	,,
,,	,,	caustic	140	,,
,,	,,	phlogisticated	142	,,
,, Zinc	,,	aerated mineral alkali	193	,,
,,	,,	caustic	161	,,
,,	,,	phlogisticated	495	,,
,, Antimony		aerated mineral alkali	140	,,
,,	,,	caustic	138	,,
,,	,,	phlogisticated	138	,,
,, Manganese		aerated mineral alkali	180	,,
,,	,,	caustic	168	,,
,,	,,	phlogisticated	150	,,

"Having compared the weights now produced, it is necessary, first, to inquire into the cause of such differences."

* * * * * Is it not then the matter of heat that is attached to the calx, and which is always united to the caustic alkali, for does it not excite heat when dissolved in the simple acids?

"This forms a triumphant foundation for assaying minerals and metals in the humid way, the mere weight of the precipitates being known. * * * * If the same mode of operating be used, the results of the experiments will be always the same. Let us say that a quantity of metal *a* in certain circumstances makes a precipitate of the weight of *b*; if the same method be used, it is obvious that *nb* may safely be allowed to correspond to *na* of the perfect metal, although in the fundamental experiment, the dissolved metal may not have been completely precipitated, or its weight may have been increased by foreign matter, still the same circumstances will produce always the same gain or loss, and the conclusion

will remain unshaken. Let then the methods be exactly decided on, and no fallacy is to be feared." *

ON ELECTIVE ATTRACTIONS.

"Simple elective attractions.† Let *A* be the substance to which *a*, *b*, *c*, &c., are drawn; further, let *A* be added to *c* to saturation, this we shall call *Ac*, when again *b* is added, let the union take place to the exclusion of *c*; then *A* is said to attract *b* more than *c*, or *b* has a stronger elective attraction than *c*; then *Ab*, when *a* is added, gives up *b*, and *a* is united instead, then it is understood that *a* excels *b* in attractive force, and the order of the efficacy of the attraction forms a series *a*, *b*, *c*. What we here call attractions, others call affinity; we use either term promiscuously in future, although the latter being more a metaphorical expression does not appear so suitable in physics.

* * * * Page 318. "It has not escaped me, that some chemists have considered, as entirely without foundation, the doctrine which asserts that neutral or middle salts can receive a distinct excess of acid. That this sometimes takes place the experiments to be related most clearly shew, although naturally it *(the excess)* adheres with far less tenacity than that portion which is necessary to effect saturation.

* * * * Page 325. "From all that has been brought forward, I consider it clear, not only that the doctrine of a decided superfluity of either ingredient is not absurd, but that in reality this result is found in many cases. Certainly this superfluity attaches itself much more loosely than the portion necessary for saturation, so that frequently it is easily driven off, but this in no way causes it to be less real."

Here we find Bergman endeavouring to obtain the amount of oxygen in metallic oxides, or phlogiston in metals. He finds that the amount in equal quantities of metals is not the same. This could only be the case if the atomic weights of all metals were the same.

* Page 396. † Vol. III., page 294.

In agreement with this, he finds that the quantity of acid necessary for dissolving certain weights of metals, differs with each metal, and the amount which one metal precipitates from the solution, differs with each metal. This was promising fair for discovery; and in the first table we have the amount of various metals needed to precipitate 100 of silver, in fact, a table of atomic weights, if he could have seen it, although imperfection in experiment rendered it difficult, and the law seemed very intricate.

He drew the conclusion that some acids dissolve metals with more oxygen in their oxides than others, when he says, 100 of silver are reduced by 31 of copper in nitric, and 30 of copper in sulphuric acid. This helped to lead him wrong.

He seems to have most naturally thought that it would be needful to find the relation of the oxygen in a metallic oxide to that in every other, and was naturally surprised at the great labour needed. We know that this would be a most complicated relationship, and that the oxygen is constantly changing its per centage relation in every compound, to such an extent, that it would be impossible to follow it without constant recurrence to its atomic weight. We may look on this inquiry of Bergman as a search, acute although unsuccessful after that last step in simplicity.

He gives a valuable discovery in the establishment of the permanence of the amount of oxygen in precipitated oxides, the very foundation of analysis, and an important step towards the knowledge of permanence of constitution in all substances whatever. That the numbers need correction, need hardly be remarked.

At the same time it seems to be beyond doubt that he did not grasp with great clearness the doctrine of permanent constitution, or he would scarcely have made these remarks on neutral salts receiving a distinct excess of acid. Any indefinite amount added, becomes a mixture only.

He extended the tables of attraction to a great length,

calling them elective attractions, preferring attraction to affinity. His tables are 59 in number, the first portion giving the wet way, and the second the dry. These were given in the old symbols, and have certainly a most formidable and unattractive appearance, in his original work. They have, however, been published in England, at an early period, in the form below.

SINGLE ELECTIVE ATTRACTIONS.

IN THE MOIST WAY.

SULPHURIC ACID.	NITRIC ACID.	MURIATIC ACID.
Potash	Potash	Potash
Soda	Soda	Soda
Baryta	Baryta	Baryta
Lime	Lime	Lime
Magnesia	Magnesia	Magnesia
Ammonia	Ammonia	Ammonia
Alumina	Alumina	Alumina
Oxide of Zinc	Oxide of Zinc	Oxide of Zinc
,, Iron	,, Iron	,, Iron
,, Manganese	,, Manganese	,, Manganese
,, Cobalt	,, Cobalt	,, Cobalt
,, Nickel	,, Nickel	,, Nickel
,, Lead	,, Lead	,, Lead
,, Tin	,, Tin	,, Tin
,, Copper	,, Copper	,, Copper
,, Bismuth	,, Bismuth	,, Bismuth
,, Antimony	,, Antimony	,, Antimony
,, Arsenic	,, Arsenic	,, Arsenic
,, Mercury	,, Mercury	,, Mercury
,, Silver	,, Silver	,, Silver
,, Gold	,, Gold	,, Gold
,, Platina	,, Platina	,, Platina
,, Water	,, Water	,, Water
,, Alcohol	,, Alcohol	,, Alcohol
,, Phlogiston	,, Phlogiston	,, Phlogiston

IN THE DRY WAY.

SULPHURIC ACID.	NITRIC ACID.	MURIATIC ACID.
Phlogiston	Phlogiston	Phlogiston
Potash	Baryta	Baryta

SULPHURIC ACID.	NITRIC ACID.	MURIATIC ACID.
Soda	Potash	Potash
Baryta	Soda	Soda
Lime	Lime	Lime
Magnesia	Magnesia	Magnesia
Metallic Oxides	Metallic Oxides	Metallic Oxides
Ammonia	Ammonia	Ammonia
Alumina	Alumina	Alumina

He gave also two tables of his famous double elective attraction, or compound attraction. The examples given are numerous, and would take too much room. The form is exactly the same as given below as Elliot's, no numbers being used.

Elliot published Bergman's tables, with the addition of figures, to show the relative force which one bore to another. He says, "suppose that (see Encycl. Method. Dict. de Chymie, vol. i., p. 552) potash and sulphuric acid attract each other with the force of 9; that oxide of silver and nitric acid attract each other with the force of 2; that the affinity of nitric acid, with potash, is 8, and that of sulphuric acid, with oxide of silver, 4. As 8 + 4 is greater than 9 + 2, decomposition takes place, and two new compounds are formed, nitrate of potash and sulphate of silver."

He then made the symbols so:—

		Nitre of Potash		
	Potash	8	Nitric Acid	
Vitriol of Potash	9		2	Nitre of Silver
	Vitriolic Acid	4	Calx of Silver	
		Vitriol of Silver		

G. Morveau continued this *schema* or *symboles*, finding new numbers, and he has put into a short table his results. This is a more definite way of showing the relation of bodies to each other than we have yet seen.

[illegible] *[illegible]finities of five Acids and* [illegible] *relations indicated by the* [illegible]

[illegible]	[illegible]	[illegible]	[illegible]	MURIATIC ACID.	ACETIC ACID.	CARBONIC ACID.
[illegible]	[illegible]	[illegible]	[illegible]	36	29	14
[illegible]	[illegible]	[illegible]	[illegible]	32	26	9
[illegible]	[illegible]	[illegible]	[illegible]	28	25	8
[illegible]	[illegible]	[illegible]	[illegible]	20	19	12
[illegible]	[illegible]	[illegible]	[illegible]	14	20	4
[illegible]	[illegible]	[illegible]	[illegible]	16	17	6
[illegible]	[illegible]	[illegible]	[illegible]	10	15	2

[illegible] to the figures, "the [illegible] no certain basis, but [illegible] number of the most [illegible] without inconvenience, [illegible]anging them, so as to [illegible]

[illegible] similar principles, but [illegible] small that it was not [illegible]he objects to Kirwan's [illegible] base as the amount of [illegible] with results.

[illegible]sition there seems to be [illegible]ch saturated one base, [illegible]

[illegible] the direction which [illegible]er of those who nearly [illegible]boured in a legitimate [illegible]heories on the subject [illegible]law.

[illegible]

[illegible]real acid in each of the [illegible] of real acid taken up by [illegible] point of saturation, led [illegible] be the true method of [illegible]

[illegible] † Page 557.

investigating the quantity of attraction which each acid bears to the several bases to which it is capable of uniting; for it was impossible not to perceive, first, that the quantity of real acid, necessary to saturate a given quantity of basis, is inversely as the affinity of each basis to each acid. 2ndly. That the quantity of each basis, requisite to saturate a given quantity of each acid, is directly as the affinity of such acid to each basis. Thus 100 grains of each of the acids require for their saturation a greater quantity of fixed alkali than of calcareous earths, more of this earth than of volatile alkali, more of this alkali than magnesia, and more of magnesia than of earth or alum, as may be seen in the following table.

Quantity of Basis taken up by 100 *grs. of each of the Mineral Acids.*

	Veget. fixed Alkali.	Mineral Alkali.	Calcareous Earth.	Volatile Alkali.	Magnesia.	Earth of Alum.
	grs.	grs.	grs.	grs.	grs.	grs.
Vitriolic Acid...	215	165	110	90	80	75
Nitrous Acid ...	215	165	96	87	75	65
Marine Acid....	215	158	89	79	71	55

"As these numbers agree with what common experience teaches us concerning the affinity of these acids with their respective bases, they may be considered as adequate expressions of the quantity of that affinity, and I shall in future use them as such. Thus the affinity of vitriolic acid to fixed vegetable alkali, that is, the force with which they unite, or tend to unite, to each other, is to the affinity with which that same acid unites to calcareous earth, as 215 grs. to 110; and to that which the nitrous acid bears to calcareous earth as 215 grs. to 96," &c.* He adds a similar table of metals and acids. Kirwan gives here what would lead to the atomic weights of the bodies had he known the law which appears to have been first published by Richter; one obtained the atomic weights as the measure of affinities, the other reciprocal

* Philosophical Transactions Abridged, Vol. XV., p. 335-6, year 1783. This was read, I believe, in 1782.

affinities, but neither knew the other's results, and both were lost sight of. The one (Ritcher) did not know that he had got close upon a universal law, the other (Kirwan) did not know that he had got the mode of expressing that universal law, but used it for what it was little worth, an expression of affinity.

We now come to Wenzel, one of those men whose names have been brought forward as a much neglected philosopher, and to whom almost every writer on the history of science, who has had occasion to mention him in later years, has been anxious to award the due honour. We see his book constantly quoted. Some writers give us his words, others give us what appears such a clear explanation of his ideas that we feel no more to be wanting. I had been long anxious to obtain his works, but after advertising in Germany, and inquiring in several towns and large libraries in this country, as well as in France and Germany, I did not obtain the volume, and proceeded without it. I afterwards found that a duplicate copy existed at the Munich Royal Library, and was fortunate enough to obtain it, duplicate copies being generally disposed of. Having read it carefully over, I found no such passages as are imputed to him; and, therefore, read it still more carefully again, and then a third time, but they did not exist. Having written to two eminent historians of science for an explanation, I find that neither had seen the volume; but one of them informed me that the mistake had been rectified in a supplement to the "Handwoerterbuch der Chemie u. Physik."*

The reciprocal saturation which results when two salts decompose each other, is the discovery, the honour of which has long been given to Wenzel. It is a curious fact that not only does he not see this, but he sees and explains the con-

* It is by Dr. J. S. C. Schweigger, and has been since published as a pamphlet (Ueber die Stoechiometrische Reihen im Sinne Richter's), &c., Halle, 1853.

trary, as he shews us that in double decomposition something always remains unsaturated; but generally very little remains. One is sorry that being so near a law, he had not the slightest conception of it. The most important part of his work, as far as our purpose is concerned, seems to me to be contained in the following sentences. The title of the work is "The Doctrine of the Affinity of Bodies."* I shall not give the original, although scarce, as the work, from the fact above stated, has lost its great importance.

In the Preface, he says, "at first my only intention was to make for my own use a treatise which should contain the order of the ascertained affinities and the circumstances under which they acted, lest I should not be able to remember them. But it occurred to me that others might find it useful also, if it were more worked out. For this end I endeavoured to explain the cause and the law of affinity on a good foundation, and the circumstances under which the bodies combine as well as the true relation of their weights towards each other.

Page 4. "It is of itself clear that any combination of bodies must have a constant unchangeable proportion, which can neither be greater nor smaller without some cause acting externally, because, otherwise, nothing certain could be decided on by comparing them. It therefore necessarily follows, that every possible combination of two bodies stands in the most exact relationship with every other, and this relation expresses the degree of combination.

Page 9. "These smallest particles of each body have at all times, in a natural state, a determinate figure; but the whole mass of the body takes a form according as chance or art gives it, without causing any change in the smallest particles, just as the tender fibres or tubes in a piece of wood remain always the same, although the whole piece may be in the shape of a ball or a cube."

* Carl Friedrich Wenzel, Lehre von der Verwandschaft der Koerper. Dresden, 1777.

Page 10. "I examine the natural structure of some metals, I see certainly nothing more than that they are hard solidly-united heavy bodies, which become liquid in the fire at different degrees of heat, and lose their former connectedness (or cohesion), and without being heavier take up a greater or less space than before. This is enough to enable us to conclude that the figure of the smallest particles of metals is changed by the fire, and that the fluid condition of the whole mass, and its altered specific gravity, are the necessary consequence of this alteration of figure. For when the mass of a body without change of weight takes up a greater or less space, it is certain that it can take place under no circumstances except a change of figure in the smallest portions of the bodies. A thousand small cubes may be put into a smaller space than the same number of spheres of the same mass and weight, and the heap made by the spheres is not so great as if they were converted into stars, and so on. When the specific gravity is altered, no matter by what means, the figure and situation of the smallest parts can no longer remain the same."

Page 20. "Besides change of figure, I know no sufficient reason for all that has been said; for if we completely banished the figure, and viewed the properties of the body as something substantial in matter, I know not how we could explain without contradiction the every-day experience; or we must, as Snellius with refraction, explain it by the will of God, which settles the matter at once; but if my understanding is to lay hold of the method by which anything acts, this explanation will not be satisfactory.

Page 28. "But we have remarked that any combination of bodies, on account of the figure of their parts, depends on static laws, and there it is proved that the motion of a weight is so much the slower the smaller the force is, in comparison with it. Let us apply this to the present case, and bodies will appear to us as so many weights, and their com-

mon solvent as a force, which acts more slowly or more rapidly on one or the other. It follows, then, that the more rapidly a common solvent unites with a body, the greater must be its degree of combination, and we obtain therefore this law.

" *The affinity of bodies with a common solvent is in the inverse ratio of the time taken to dissolve.*

Page 31. " We have now a universal law, according to which the affinity of bodies or their rank in the series is decided, and we obtain at once this important advantage, that we not only know that the union of a common solvent is greater or less with any body, but also how much greater or less it is, because the difference of the time of solution shews the difference of the combination. Therefore amongst a number of bodies, the combination of one with a common solvent may be considered as a quantity which may be expressed by a fixed number, if we take the smallest in such a series as *unity*; and by this means we are able to give a correct explanation of all phenomena.

Page 46. " This important question, then, remains, why a solvent, when it is only moderately diluted, does not in the least attack certain metals, but as soon as another metal is dissolved in it, with which it naturally has a less affinity, a ready solution of the first takes place. Page 47. Because here the powers meet which assist each other.

Page 72. " The circumstances under which this metal (iron) is dissolved by vitriolic acid are these, that the acid must not be strong. When both unite, iron vitriol is formed, which loses the most of its acid in the fire, as well as by frequent solution in water. A small bored cylinder of Styrian steel of 102 grains was put into half an ounce of the spirit of vitriol, diluted with an equal quantity of water, exactly as with the zinc experiments; there remained 46¾ grains of steel, and 55¼ grains were dissolved in the half ounce of the spirit of vitriol.

"Therefore the relation of the hardest steel to the strongest vitriol is 175 : 240.

"*Application of the doctrine of affinity of bodies.** "This will best be shewn by examples.

"Is it possible by Beguin's spirit of sulphur (a sulphide of ammonium chiefly) to decompose the *luna cornua*, or to separate the muriatic acid entirely without loss?"

"To settle this question we require only the following experiments. Muriatic acid has a smaller degree of affinity with silver than with the volatile salt. Sulphur, on the other hand, unites with silver in preference to the volatile salt. The silver is not separated from the muriatic acid by the volatile salt, on account of accidental circumstances, but this separation follows the moment any other body unites with the silver, if it has not the property of dissolving the silver. But sulphur is just such a body, and is, therefore, fitted for the purpose. If, then, the spirit of sulphur of Beguin is poured on finely powdered luna cornua, it is easily seen that the muriatic acid in the luna cornua must unite with the volatile salt in the spirit of sulphur, and the sulphur will unite with the silver. The new products that are formed by this separation, can consequently be nothing more than common sal ammoniac and sulphuretted silver.

Page 452. "Another similar question arises by which the proportions of the mixture must be considered. How much cinnabar must be mixed with the *luna cornua*, so as completely to separate the silver?

"The possibility of this decomposition may be shewn in the same way as in the first case. If no particular experiment is made, it depends on the comparison only of the following propositions. Half an ounce of *luna cornua* contains $180\frac{9}{16}$ grains of fine silver. Half an ounce of fine silver takes up $35\frac{1}{2}$ grains of sulphur. We may then calculate that for $180\frac{9}{16}$ grains of fine silver, $26\frac{3}{4}$ grains of sulphur are required. We know besides,

* Page 450.

that cinnabar contains sulphur in the proportion of 65 to 240 of quicksilver, or 65 grains of sulphur united with 240 of quicksilver, are to be met with in 305 grains of cinnabar, therefore $26\frac{3}{4}$ grains of sulphur are contained in $125\frac{1}{2}$ of cinnabar. This quantity of cinnabar, as regards its sulphur, will be sufficient for the decomposition of half an ounce of *luna cornua*.

"But we must inquire if $125\frac{1}{2}$ grains of cinnabar contain as much quicksilver as will be sufficient to take in the muriatic acid which is saturated with the silver. Half an ounce of luna cornua contains $53\frac{7}{16}$ grains of muriatic acid of greatest concentration. In half an ounce of the caustic sublimate there are $58\frac{1}{3}$ grains of the strongest acid, which is saturated with 174 grains of quicksilver. From this proportion it is found that $53\frac{7}{16}$ grains of the strongest muriatic acid are required for $159\frac{2}{5}$ grains quicksilver. Now as there are in cinnabar 240 grains of quicksilver united with 65 grains of sulphur, $159\frac{2}{5}$ grains of quicksilver require $43\frac{1}{6}$ grains of sulphur. Both together give nearly $202\frac{1}{2}$ grains of cinnabar. Consequently, from $125\frac{1}{2}$ grains of cinnabar, all the muriatic acid found in the luna cornua is not separated. We see from this that the muriatic acid of the lunar caustic rises in sublimation with the quicksilver out of the $202\frac{1}{2}$ grains of cinnabar as a caustic sublimate, whilst the silver remains united only with so much sulphur as it found in $125\frac{1}{2}$ grains of cinnabar."

His ***smallest parts*** of bodies are not atoms, but molecules rather, or particles, as they change their form.

He has made a theory of affinity, and attempted to represent the force by a number. To attempt to give the numerical or dynamical ratio of every body to each other was an object of the very highest kind, and we must look on him as one of those less fortunate men who, when search was required in every direction, has had the misfortune to have the wrong one assigned to him. He searched in the direction of time, and obtained a manifest fallacy; as bodies are constituted abstractly

he might be correct, but his theory cannot be introduced into science at present, and in the way he introduced it, it was entirely a mistake. But he has done great service in early times in seeking for the distinct constitution of bodies, and in asserting the constancy of combination; whilst he obtained numbers representing the constant relation of bodies to each other, he failed to see that they would be reciprocal. This failure at once removes him from the great discoverers, and places him among those honourable and valuable labourers in science whose names are read with respect by students, but who cannot be recognised by mankind generally, because the capacities of our minds are too small to retain more than the lives of a few of the most eminent.

The doctrine of reciprocal proportion must be taken from him, and he can now no longer hold a place in the history of the atomic theory other than as the author of an intelligent attempt which has entirely failed.

I feel sorry to leave him in this state, and a few kind words will do little good. I believe he would have preferred the truth; the honour he received was not required by him; the discovery was not claimed by him; he died in 1793, before it was known to be worth making. In his works he appears an honest, earnest man.

CHAPTER VIII.

DR. BRYAN HIGGINS AND WILLIAM HIGGINS.

DR. BRYAN HIGGINS was the earliest chemist who seems to have dealt with the elements of matter, in a chemical, as well as physical sense, with an attempt to obtain information as to the primary elements and their atomic constitution, although he gives no certain results. I find at the laboratory of Owens College, among the books forming part of the library of the late Dr. William Henry, a little pamphlet, proposing a course of lectures, to be commenced on the 13th November, 1775.

The proposals are said to have been formerly published; it is not said how long before. I shall give considerable extracts.

Page 1. "Doctor Higgins, of Greek-street, Soho, encouraged by the literary noblemen, and gentlemen who have subscribed to his courses of philosophic and practical chemistry, addresses the following proposals to the patrons of natural philosophy and useful arts.

"That fifty philosophic and literary gentlemen do concur in promoting experimental inquiries into the elements of matter and laws of nature, and such other subjects as are most important in natural philosophy, chemistry, and arts.

Page 3. "That in these discourses he shall introduce the natural phenomena, the illustrative observations and experiments of philosophers, chemists, and artists; and particularly his notions and experiments concerning the primary elements and the properties of matter.

Page 9. "Introductory discourse on matter in general, called gross matter; on the varieties and distinctions of gross matter; on the primary elements of matter.

"Observations on the experiments and philosophy exhibited in the foregoing course of chemistry, and other experiments which demonstrate the existence of seven primary distinct elements of matter, viz.;

Earth,	Air,
Water,	Phlogiston,
Alkali,	Light.
Acid,	

"Experiments, observations, and arguments, persuading that each primary element consists of atoms homogeneal; that these atoms are impenetrable, immutable in figure, inconvertible, and that, in the ordinary course of nature, they are not annihilated, nor newly created.

"Observations and experiments, persuading that the atoms of each element are globular, or nearly so, and that the spiral, spicular and other figures ascribed to these atoms, are fictions unnecessary, and are inconsistent with the uniformity and simplicity of nature, and repugnant to experience. * * *

"Experiments and observations showing that the possible and known unions of the foregoing elements, and that the possible and known proportions in which the unions of the foregoing elements may take place, are more numerous than the bodies distinguished by philosophers and naturalists; persuading that all known bodies are really composed of one or more of the foregoing elements; and that all bodies must be admitted to consist of these only until other elementary matter is found necessary for the explication of the natural phenomena, and is demonstrated to exist.

"A classical arrangement on the table of bodies composed of two or three primary elements, which bodies, in various chemical processes, not being decomposed, we call chemical elements, or the elements of the chemists.

"A like classical arrangement of bodies composed of two chemical elements.

"A like classical arrangement of bodies and natural substances composed of many chemical elements.

OPINION,

"1. That the homogeneal atoms of five elements repel reciprocally.

"2. That the homogeneal atoms of two elements attract reciprocally.

"3. That the dissimilar atoms of five elements attract reciprocally.

"4. That the dissimilar atoms of two elements repel reciprocally.

"That the attraction subsisting between elementary atoms is more forcible in one direction or axis of each atom than in any other direction, and that there is a polarity in all matter whatever.

"6. That there is but one species of attraction operating with great force between the similar or dissimilar atoms of certain elements, and with less force between those of other elements, in gradations; but in all affected by distance and polarity.

"7. That the attraction of bodies enumerated as distinct properties of matter or laws of nature, are nothing more than the sums of the attraction of their elementary atoms, or these forces concentrated in a certain degree by the pressure of repellent atoms, or these forces exerted to the greatest advantage in bodies whose primary elementary attractions are strongest, and whose primary elementary atoms are also arranged in polar order.

"8. That specific gravity is not as the quantity of matter in a given space, but as the quality of the matter, or the sum of its elementary attractions; consequently, that light bodies are not necessarily more porous than the heaviest."

Page 14. "Observations and experiments, showing the grounds on which we ought for a while to admit the following distinctions of earths, viz:

"Seven earths, capable of forming ductile metals.

"Seven earths, capable of forming metals not ductile.

"Seven earths, incapable of forming metal.

Question 1. Is there but one earthy element, which, in various modes of aggregation, or in indissoluble combination with other elementary matter, forms twenty-one earthy bodies? or, Question 2, Are there three times seven, or seven times seven earthy elements?

Page 15. "Experimental and geometrical estimation of the force of this attraction in fortuitous arrangement of the atoms, and of the force of this attraction in the polar arrangement of the atoms.

On attraction he says, page 23, "That no element doth saturate, nor can saturate, the like element; that no element, whose atoms attract each other, can saturate any other element whose atoms attract each other; that a repellent element doth saturate non-repellent elements, and *vice versa;* that repellent elements do saturate reciprocally; and that attraction and repulsion, operating adversely, are the cause of saturation; and saturation is not a distinct or primary law of nature, but an effect."

We find here no ideas given of definite compounds, except so far as the ordinary idea of saturation is concerned. Had Dr. Higgins any theory resembling the present atomic theory, he would certainly not have expressed himself so darkly on the subject of saturation.

We even find that he is not quite freed from the *prima materia,* although he restricts it to the matter of certain classes, such as the matter of earth forming many earths, the acid matter forming many acids. He is, therefore, to be viewed as one bordering on the transmutation theory, not freed from mystic ideas, but grappling with the subject so energetically, that in some directions he almost sees his way into another region of theory.

I shall quote the most important sentences in the "Experi-

ments and observations"* of Dr. Higgins. In distilling acetate of lead, he found a certain amount of what he calls acid matter in the fixable air, which he considers, as before mentioned, to be a peculiar principle. It combines with the empyreal air (oxygen) of the litharge. Not accounting for the whole amount by the measurements he made, he inferred "that when the acid matter of acetous acid is employed in excessive quantity to form fixable air with the empyreal air of litharge, the fixable air may consist of a little more than one part of the acid matter, combined with two of the empyreal air. By a more accurate estimate of the fixable air taken at 85 grains, it is most probable that the proportions would be found to be accurately two to one, provided fixable air, like other acids, may not subsist with various proportions of the empyreal air." †

He mentions the definite proportion, two to one, because he obtains the figures approximatively so, but he not only fails to elevate it into a principle, but speaks of various proportions as probable in the case in hand, and as usual in other cases. This seems equal to saying distinctly that he recognised no such principle. But does the term "various proportions" allude to fixed numbers, such as two or three? There is no reason to suppose this, he uses the word in the ordinary sense it was then used, no other sense had been given to it, and any other sense in this place is impossible.

When firing with oxygen the inflammable gases from acetate of lime, he says, in reference to some experiments unnecessary to be detailed, "by other experiments and the same kind of estimation, the empyreal air appeared to constitute more than two-thirds of the fixable air, and in some it seemed to be accurately two-thirds; but after all I continued

* Experiments and observations relating to acetous acid, fixable air, dense inflammable air, oils and fuel. The matter of fire and light, metallic reduction, combustion, fermentation, putrefaction, respiration, and other subjects of chemical philosophy. By Bryan Higgins, M.D. London, 1786.

† Page 232.

to suspect that the proportions of the principles are not the same in every specimen of the elastic fluid which we consider as fixable air. When the dense inflammable air first expelled from charcoal was used, the result was nearly the same. And from the whole, I conclude, that when as much inflammable air of this kind is employed, as can be converted in the explosion, by the empyreal air, the fixable air consists of one part by weight of the acid matter of acetous acid or charcoal, and nearly, or accurately, two parts of empyreal air; and almost one-fifth of this kind of inflammable air is phlogiston, and the remainder mere acid basis of fixable air, and of acetous acid, charcoal, oils, spirits, and of all substances that yield acetous acid or dense inflammable air abundantly."*

The same remarks apply to this. The "nearly" two parts are just as probable, in his mind, as the "accurately" two parts, which would not have been the case had he found any defining law to express on the subject.

Then he says, further on,† "It seems, therefore, that the proportions in which the acid and phlogistic matter are combined, in different specimens of inflammable air expelled from vegetable substances, do vary considerably," &c. This refers to his tables of results.

He believes that the particles of the different gases unite to form molecules of compound gases, page 317: "I consider the specific gravity as a safe guide in our investigation of these affinities and of their order, in regard only to the elastic fluids which seem to consist of no more than one kind of gravitating matter engaged in the repellent atmospheres; and of fixed air, dense inflammable air, acid air, the phlogistic alkaline air, and others, I would observe, that the atmospheres include molecules, instead of solitary ultimate parts; for, without this chemical union of heterogeneal parts, and the formation of molecules, an elastic fluid of the kind that I now speak of could not differ, as it does, from either kind of matter

* Page 201. † Page 204.

of which it is composed. From this consideration of the attractive forces which tend to form molecules, and of the atmospheres which, in compound elastic fluids, encompass the molecules, but not the ultimate parts severally, we derive an easy explanation of the phenomenon so often noted in the preceding pages—I mean, the conversion of a substance, not into air, but into two or three different elastic fluids, by mere ignition."

Dr. Higgins thinks of atoms, of simple particles, and even speaks of gases uniting, in some cases, in nearly, if not accurately, a fixed proportion, and yet he sees no law. He does not carry his idea far enough. If the molecules are formed by the union of two particles the proportions must of necessity be fixed, or if any number of particles unite, the proportions are fixed, unless the molecules are to be supposed of different constitutions. In this last case, they would constitute a mixture of different gases. But it was not known at this period that all bodies had a fixed constitution, otherwise one would have supposed that Dalton's laws would have been readily arrived at by Dr. Higgins. On the other hand, his theory was not clear, or he would have been led by it to decide on the necessity of fixed constitution as a result. But we obtain no results affecting chemical philosophy. And yet amongst other questions which he says "we shall find no difficulty in answering," is, "Why does any excessive quantity of empyreal or of inflammable air, beyond the determinate proportions in which their gross parts can combine, remain elastic and unaltered, or not altered in any considerable part of it, after the combustion?" *

The nearest answer he gives as to the proportions is the following:—"The matter of fire limits the quantity in which aeriform fluids, and bodies containing it, can combine chemically." † We may conclude then that nothing in the gases themselves determined the proportion, but the cause was in "distinct atmosphere of fiery matter."

* Page 330. † Page 307.

In reading the first part of this question we suppose he must live as a discoverer; in reading the second part, we find him timidly committing suicide. He wavers between his theory and his experiments. If determinate proportion be a law, he might have supposed it rigid, like other laws of nature; but he leaves a portion of the residue altered, so that, after all, we only receive from him indeterminate, or nearly determinate proportions.

The experiment to which the question refers occurs in p. 296. He draws no valuable conclusion from it. He says also, p. 299, "In the combustion of charcoal with empyreal air, the expenditure of the latter, in fixable air and water, was always found to be more than thrice the weight of the charcoal. I could now easily ascertain the proportion of these, and even the quantity of the acid and phlogistic matter in this and other bodies; but as my present purposes are answered by approximation, I think it unnecessary to detain the reader any longer on this subject."

His principal object was to explain the nature of fire, which he considers as subject to the laws of gravitation, and to be the cause of the aeriform state of bodies.

The only law in which he introduces numbers is the fourth of his "primary notions of the matter of fire." "The changes of repellent matter, by which attractive and gravitating particles form elastic fluids, are distinct atmospheres of fiery matter, in which the densities are reciprocally as the distances from the central particles, in a duplicate or higher ratio." *

His writings are mostly in the first stage of thought before opinion is formed. Commentators on such persons are obliged to extend the ideas a little in order to make them clear, and so the original writer gets credit for more than he had ever done. Such writers are of great value when they lead towards discoveries; but we are apt to give them the entire honour when

* Page 306.

they deserve only a part. As far as our subject is concerned Dr. Higgins has small claims. His opinions on atoms might have been held by the ancients: whilst standing on their shoulders, it would have required much less sagacity to discover than was needed for them. He speaks of the sums of the forces of atoms measuring the attraction of matter, but does not suppose that if matter be atomic, the number of atoms might also, in this way, be got comparatively.

We might say, however, that Dr. Higgins began the study of atomic chemistry, properly so called, although he made little advance in it.

The next person worthy of note is of the same name and family. The following extracts contain what is of greatest importance in connection with our subject. The title page of the volume quoted from is, "A Comparative View of the Phlogistic and Antiphlogistic Theories, with Inductions, &c., by William Higgins, of Pembroke College, Oxford. The Second Edition.

"Est quoddam prodire tenus, si non datur ultra.

"London: Printed for J. Murray, No. 32, Fleet Street. 1791." The first edition was in 1789.

Page 14. "It is generally allowed, and justly, that nitrous air consists of dephlogisticated air and phlogistic, in the proportion of two of the former to one of the latter. The supposition of its containing phlogiston, I hope, will hereafter appear to be erroneous; therefore, every ultimate particle of phlogisticated air must be united to two of dephlogisticated air; and these molicules, combined with fire, constitute nitrous air. Now, if every (one) of these molicules were surrounded with an atmosphere of fire equal in size only to those of dephlogisticated air, 100 cubic inches of nitrous air should weigh 98,535 grains; whereas, according to Kirwan, they weigh but 37 grains. Hence, we may justly conclude that the gravitating particles of nitrous air are thrice the distance from each other that the ultimate particles of dephlogisticated

are in the same temperature, and, of course, their atmospheres of fire must be in size proportionable; or else some other repelling fluid must interpose. The size of the repelling atmospheres of nitrous air thus considered, and likewise the weaker attraction of the molicules of this air to dephlogisticated air than that of the ultimate particles of phlogistic in their simple state, it is surprising to me, with how much more facility the former unites to dephlogisticated air than the latter."

After speaking of the combustion of sulphur, he says, p. 35, "A good many more facts might be urged on this subject; but, in my opinion, enough has been adduced to convince an impartial reader that all the phenomema above recited are only explicable by entirely leaving out phlogiston, and supposing sulphur to be a simple subtance, whose ultimate particles attract dephlogisticated air with forces inherent in themselves, independent of phlogiston or concrete inflammable air, as an alkali does an acid, or gold and tin mercury; and likewise supposing the combustion of sulphur to be as simple a process as that of light inflammable air; that is, that there is no dephlogistication, or formation of water, during the union of the oxygenous principle to sulphur, as containing not a particle of light inflammable air in its constitution. I have often combined sulphur rendered perfectly dry, and dephlogisticated air likewise, deprived of its water by fused marine selenite in large proportion over mercury, and could never observe that water was produced. Indeed it may be said that the volatile sulphurous acid, which is always the result of this process, may redissolve it; but this is not very likely, when a small portion of water will deprive it of its elasticity."

"According to Mr. Kirwan, 100 grains of sulphur require 143 grains of dephlogisticated air to convert them into volatile vitriolic acid; but they require much more in order to become perfect vitriolic acid. Highly concentrated vitriolic acid contains two parts of dephlogisticated air, and one of

sulphur, exclusive of water. One hundred and forty-three grains of dephlogisticated air contain 41 of water, for lime will abstract 26 grains from it, and the remainder cannot be separated from it in its aerial state; therefore 100 grains of sulphur, making an allowance for water, require 100 or 102 of the real gravitating matter of dephlogisticated air to form volatile vitriolic acid; and as volatile vitriolic acid is very little short of double the specific gravity of dephlogisticated air, we may conclude that the ultimate particles of sulphur and dephlogisticated air contain equal quantities of solid matter; for dephlogisticated air suffers no considerable contraction by uniting to sulphur in the proportion merely necessary for the formation of volatile vitriolic acid. Hence we may conclude, that, in volatile vitriolic acid, a single ultimate particle of sulphur is intimately united only to a single particle of dephlogisticated air; and that, in perfect vitriolic acid, every single particle of sulphur is united to two of dephlogisticated air, being the quantity necessary to saturation."

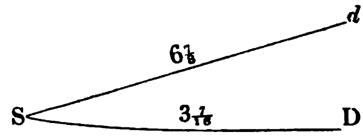
"As two cubic inches of light inflammable air require but one of dephlogisticated air to condense them, we must suppose that they contain equal number of divisions, and that the difference of their specific gravity depends chiefly on the size of their ultimate particles; or we must suppose *that the ultimate particles of light inflammable air require two or three, or more, of dephlogisticated air to saturate them. If this latter were the case, we might produce water in an intermediate state, as well as the vitriolic or the nitrous acid, which appears to be impossible; for in whatever proportion we mix our airs, or under whatsoever circumstance we combine them, the result is invariably the same.* This likewise may be observed with respect to the decomposition of matter. Hence we may justly conclude, that water is composed of molicules formed by the union of a single ultimate particle of dephlogisticated air to an ultimate particle of

light inflammable air, and that they are incapable of uniting to a third particle of either of their constituent principles. The above notions of water and vitriolic acid being strictly kept in view, let us now proceed to inquire into the nature of sulphur and vitriolic acid, and their various effects on different bodies in the antiphlogistic doctrine. It has been already observed that metals attract dephlogisticated air with greater force than sulphur, and that sulphur attracts it with greater force than light inflammable air. It has likewise been observed, that vitriolic acid and water, mixed in a certain proportion, will calcine metals with greater facility than concentrated vitriolic acid, and that water will have very little effect on metals in a common temperature. These facts, though they may appear contradictory in themselves when slightly considered, may be accounted for on the following principles, and are, in my opinion, inexplicable by any other means whatever."

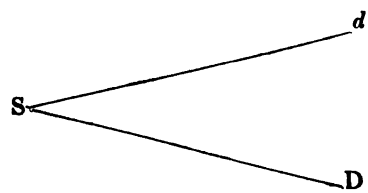
"Let us suppose iron or zinc to attract dephlogisticated air with the force of 7, sulphur to attract it with the force of 6 7-8ths, and light inflammable air with the force of 6 5-9ths. Let us again suppose these to be the utmost forces that can subsist between particle and particle. That is to say, in water dephlogisticated air is retained with the above force, and likewise in volatile vitriolic acid, with the force already mentioned. It is unnecessary to introduce here the aggregate attraction which frequently preserves a neutrality between bodies, as, for instance, between water and zinc, or water and iron. Stating the attractive forces in the above proportion, which I am led to believe is just from facts already observed, we should imagine that iron or zinc would calcine in water with greater facility than in vitriolic acid; and if some other circumstances did not interfere, it must be the case. This the following will in some degree illustrate.

Let S be a particle of sulphur, *d* a particle of dephlogisticated air, which it attracts with the force of 6 7-8ths, and let the compound be volatile sulphureous acid; here the tie be-

tween S and *d* is greater by 2-8ths, than that between the constituent principles of water, which is but 6 5-8ths. As the attraction of bodies is mutual, let us suppose S to possess one-half of this force, which is 3 7-16ths, and this to be its utmost exertion, and likewise *d* to possess the other half, which is 3 7-16ths more, which will unite them with the above-mentioned force. Let us suppose another particle of dephlogisticated air D to have a tendency to unite to S, with the force of 3 7-16ths, in order to form perfect vitriolic acid; to receive D, S must relax its attraction for *d* one-half. That is, the force of 3 7-16ths will be divided and directed in two different points, which will reduce the attachment of dephlogisticated air and sulphur in perfect vitriolic acid to 5 1-18th."



Page 66. "When vitriolic acid, whether diluted or not, is mixed with oil, an ultimate particle of vitriolic acid influences with a certain force an ultimate particle of oil, while the latter attracts the vitriolic acid with the same force. The oil will not take D *d* from S; but from the joint attraction of S —— D—— *d* to oil, they will approach with equal pace, and combine. Thus this mixture more than mechanically, but not quite chemically united, may be resolved into the different fluids mentioned above. The particle of oil will retain D or *d*, and form fixable air; at the same time that S will retain *d* or D with its full force, and form volatile vitriolic acid."

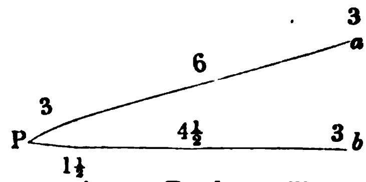


Page 132. "In my opinion the purest nitrous acid contains 5 of dephlogisticated to 1 of phlogisticated air. Nitrous air, according to Kirwan, contains 2 of dephlogisticated to 1 of

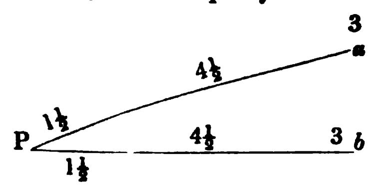
phlogisticated air. According to Lavoisier, 100 grains of nitrous air contain 32 grains of phlogisticated air, and 68 of of dephlogisticated air. I am myself of the former philosopher's opinion; I likewise am of opinion, that every primary particle of phlogisticated air is united to two of dephlogisticated air, and that these molecules are surrounded with one common atmosphere of fire."

"To render this more explicable, let us suppose P to be an ultimate particle of phlogisticated air, which attracts dephlogisticated air with the force of 3; let *a* be a particle of dephlogisticated air, whose attraction to P we will suppose to be 3 more, by which they unite with the force of 6. The nature of this compound will be hereafter explained.

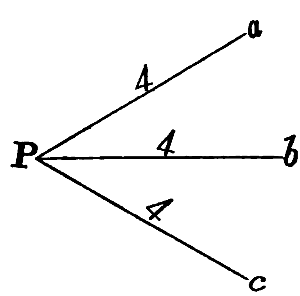
"Let us consider this to be the utmost force that can subsist between dephlogisticated and phlogisticated air. Let us suppose another par-



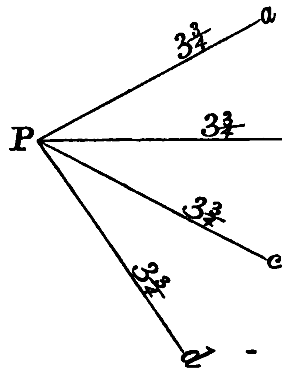
ticle of dephlogisticated air *b* to unite to P, they will not unite with the force of 6, but with the force of 4½; that is the whole power of P, which is but 3, will be equally divided and directed in two points towards *a* and *b*; so that P and *a b* will unite with the forces annexed to them; for the attraction of *a* and *b* to P



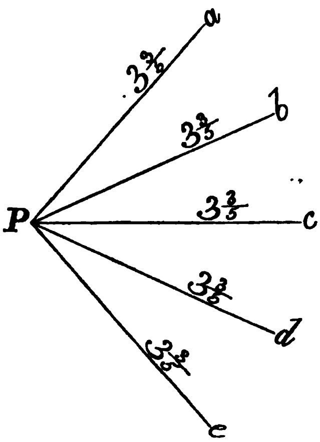
meeting with no interruption, will suffer no diminution. This I consider to be the true state of nitrous air. Let us now suppose another particle of dephlogisticated air *c* to unite to P, it will combine only with the force of 4, whereby *a*, *b*, *c* and P will gravitate toward one another. Such is the state of the red nitrous vapour or the red nitrous acid.



"Let us again suppose a fourth particle of dephlogisticated air *d* to combine with P, it will unite only with the force of $3\frac{3}{4}$. This, I think, is the state of the pale or straw coloured nitrous acid.



"Lastly, let us suppose a fifth particle of dephlogisticated air *e*, to unite to P, it will combine with the force of 3 3-5ths, so that *a*, *b*, *c*, *d* and *e* will each gravitate towards P as their common centre of gravity. This is the most perfect state of colourless nitrous acid; and in my opinion no more dephlogisticated air can unite to the phlogisticated air, as having its whole force of attraction expended on the particles of dephlogisticated air, *a*, *b*, *c*, *d*, *e*. *This illustrates the nature of saturation*. Thence we find that dephlogisticated air is retained with less force in the perfect or colourless nitrous acid, than in the straw-coloured, or in the red, or in nitrous air."



Page 194. "If the calcination of metals depended solely upon their union to dephlogisticated air, it must be supplied by water, when steam is brought in contact with them; and as every particle of light inflammable air is united but to a single particle of dephlogisticated air, inflammable air must be disengaged in proportion to the quantity of dephlogisticated air which unites to the metals; or, in other words, according to the degree of calcination it acquires."

Higgins wrote on phlogiston, his chief object being connected with it. In an octavo volume of above 300 pages these are nearly all the extracts relating to this subject. We find, then, that he did not establish a law in connection with these ideas. We must conclude, then, that he did not see their importance, that he did not see their application. Not only so, but being lost amongst so much material, we do not find that they were so written as to draw any attention, nor does he seem to have wished to do so. He wished to draw attention to what was in reality of less importance, his objections to phlogiston. The magic sword that would have slain all his enemies he threw away as some common truncheon. The magic lamp that would have given brilliance to him and to science, was found, after some years, rotting in his cellar. As regards the effect his book had on his cotemporaries, Dr. Thomson, of Glasgow, is the best authority. We see from him that no idea of an atomic theory was got from Higgins. His opinions were given and received as a speculation more than as expressing a fact or a law.

He says in his Annals of Philosophy, May, 1814, Vol. III., p. 331, "I have certainly affirmed that the atomic theory was not established in Mr. Higgins's book. And here is my reason. I have had that book in my possession since the year 1798, and had perused it carefully; yet I did not find anything in it which had suggested to me the atomic theory. That a small hint would have been sufficient I think pretty clear from this, that I was forcibly struck with Mr. Dalton's statements in 1804, though it did not fill half an octavo page; so much so, indeed, that I afterwards published an account of it; and I still consider myself as the first person who gave the world an outline of the Daltonian theory."

This is put too strongly. Had Dr. Thomson paid as much attention to Higgins's book as to the remarks of Dalton, he would certainly have made a great advance on

the chemists of the period, by understanding definite proportion, and learning to reason in the spirit of the atomic theory.

William Higgins made an advance on Bryan Higgins in this theory of sulphur and heat, and he was a man evidently of an acute mind. But he was destined to find Emerson's saying true, that we often find in the sayings of great men our own rejected ideas. He was heir to the common opinion that atoms existed, and the opinion of Dr. Higgins that they united and formed molecules of compound bodies. He applied the reasoning further, and said that they must then unite in numbers of one or two, or three, and that there could be no intermediate combination, as there were no intermediate division of atoms. He applied this reason in two or three cases. These cases, such as nitric acid, are so clear and beautiful, that we can only be surprised that the general law was not seized on. They are the first clear and satisfactory reasons given for saturation, and for definite proportion in general. Higgins was therefore the first man who used the idea of atoms with such force as to be serviceable in chemistry. He used the idea of ultimate particles and the molecular state of bodies to illustrate saturation, and definite and multiple proportion, and gave us therefore the fundamental ideas of stoechiometry as they exists in chemical science, from which everything else might have easily flowed.

He had seen the right road, but dared not go farther. But we must take his own apology, "Est quoddam prodire tenus, si non datur ultra." It is something to have gone thus far, if he had no power to go beyond it.

Like Dr. Higgins, and the most of the chemists of last century, he assumed certain forces of attraction, and endeavoured to give comparative values to the forces of combination. These numbers representing affinity misled Higgins. Had he seen any general law, he would have seen that weights repre-

sented the comparative number of his atoms, but this he did not see with any distinctness, or he would have used Kirwan's numbers, which he knew well enough, and would have found in them atomic weights. But atomic weights were never thought of at all. It was not even certain then that all matters gravitated, although he held the present opinions on it. He reasoned to a certain extent in the true spirit of the atomic theory, but becoming entirely involved in dynamics, he entirely missed his way. Dynamics have hitherto entirely failed in chemistry. Their power has been used in upsetting their friends, and Higgins fell a victim to their forces.

Having for a moment laid hold of the idea that bodies which are atomically constituted must be formed of the union of one or more bodies, and of no intermediate atoms, he drew insufficient conclusions, and all its prospective advantages were lost to him. The want of suitable results, which it was his fault for not finding, seems to have caused him to let go his magic weapon, or to view his own opinion as a speculation. In his mind they exist as very little more.

I look upon him as the first man who ever in his imagination formed a correct atomic compound, and gave a correct analysis, in spite of the thousands of previous speculations and the simplicity of the idea, but one who lost the opportunity of elevating his idea into a great law of nature. It is well to express the claim of a discoverer in the widest and in the fewest words. He expressed the fact of atomic simple and multiple proportion, which is the foundation for all the other atomic laws, although in his mind it was not raised to the dignity of a great law, and it is for great laws only that we can give great honours in this case.

Higgins speaks so clearly and simply that we can readily believe that he would have illustrated the laws of chemical combination with great beauty had he seen the great value of his ideas. There is no obscurity in his language; there is no

difficulty in telling exactly his place in science; but there is a difficulty in defining it exactly when we have to deal with Dalton, who grasped the whole so much more firmly, enlarged it, placed it, and established it in a series of laws. Any one to be put before Higgins must have made great advances: he cannot be put down by any obscure sentences dragged from any author. Any one to eclipse him must be fuller, more decisive, and more systematic.

From the want of these qualities Higgins appears more in the character of a great thinker than of a great discoverer.

CHAPTER IX.

RICHTER.

DURING the disputes as to Dalton's priority of discovery, it was frequently asserted that his atomic laws were not new, and they were, as is usual in such cases, attributed to various persons. Of these persons, Higgins, in this country, and Richter, in Germany, have been the most prominent. I have endeavoured to show exactly the position of Higgins; I shall do the same with Richter. Higgins came first with clearness and simplicity, uttering a beautiful idea which he failed to follow up; Richter came close after him, with great labour and enthusiasm, filled with a great idea of the study in which he was engaged, and obtained a law which he failed to follow up; he lost himself in complicated theories, having no idea how simple was the truth he sought for. Both were neglected, as happens when men fail to give completeness to their inquiries, even in the eyes of those who study and are willing to learn.

Richter's books are—" Anfangsgründe der Stoechyometrie oder Messkunst Chymischer Elemente." 3 vols. Bresslau und Hirschberg, 1792-4; and " Ueber die Neuern Gegenstände der Chymie," 1791-1802.

I shall give rather copious extracts from his works, shewing the direction of his inquiries, and the ends he attained.

RICHTER, VOL. I., PREFACE.

" Mathematics includes all those sciences which refer to magnitude, and consequently a science lies more or less in the province of mathematics (geometry), according as it requires the determination of magnitudes. In chemical experiments

this truth has often led me to the question, whether and how far chemistry is a part of applied mathematics; and especially in considering the well-known fact, that two neutral salts, when they decompose each other, form again neutral compounds. The immediate consequence, in my opinion, could only be, *that there are definite relations between the magnitude* of the component parts of neutral salts. From that time I considered how these proportions could be made out, partly by exact chemical experiments, partly combining chemical with mathematical analysis. In my inaugural dissertation, published at Königsberg, in 1789, I made a slight attempt, but was not then supplied with the requisite chemical apparatus, nor was I sufficiently ready with all requisite information, bearing on my present system, imperfect as it may be. The result, therefore, was very imperfect. I promised, however, not to let the matter rest with that imperfect essay, but to work out this branch with all the accuracy and profundity of which I was capable, as soon as I was supplied with the requisite conveniences. This promise, I hope in the present volume, to make good, although I am far from believing that what I am now going to say will not be in need of still more thorough and accurate elaboration, for who will venture to limit the extent and the power which is the destination of a young and budding science."

He was the first to speak of a science of stœchiometry, and began formally to lay the foundation. We may even say that he commenced the systematic study for which he gave us also the most appropriate word. I cannot say that he began the science, and it will be seen that his mode of inquiry was wanting in directness and his results in completeness.

In page 29 of the preface, he says, "as the mathematical portion of chemistry deals in a great measure with bodies which are either elements or substances incapable of being decomposed and as it teaches also their relative magnitudes,

I have been able to find no more fitting name for this scientific discipline than the word stœchiometry, from *στοιχειον* which, in the Greek language, means a something which cannot be divided, and *μετρειν* which means to find out relative magnitudes."

Here, then, is a man prepared for the work, one who resolutely laboured for many years to find the law by which the elements combine, by "number, weight, and measure."

We have seen already that many facts were known, and that even reciprocal proportion was almost attained in the diagrams which have been given, and that the most far-sighted chemists saw the natural necessity for a constant proportion in combinations; but when the well-known laws agreed upon by chemists were put together, we see how few G. Morveau's list amounted to.

Richter did a great deal of work, especially in connection with the chemistry of the metals, but everything was held secondary to his great idea of definite proportionate quantities (bestimmte Grössenverhältnisse). On the title pages of the various papers or parts of volumes, written after his stœchiometry, he has preserved as a motto "*Παντα* (ΘΕΟΣ) *μετρω και ἀριθμω και σταθμω διεταξε*" (*ας*). This, from the 'Wisdom of Solomon,' chapter xi., v. 20, is exceedingly appropriate, but the context evidently shows that it was not applied to any such subject; at the same time it is introduced as a proverb would be, or a well-known universal law coming in aptly to illustrate one particular point to which it bears no more intimate relation than to innumerable others. It must, however, be confessed, that this expression is given with a minuteness and fulness which warrants the conclusion that it was not uttered until after many and profound speculations on the order of creation. The sentence is the expression of the circumstances in all their fulness, but like many other sentences of antiquity, the meaning is not clear till the facts have been discovered piece by piece.

The most important sentences bearing on the subject of Richter's volumes have been selected, including everything which seems to indicate any knowledge of the subject. His prolixity is excessive, every little idea is long dwelt upon, and as an example of the small fear he had of too much enlarging his book, it may be stated that he actually writes a system of algebra in one of the volumes, because a little algebra is wanted for the full understanding of his demonstrations. It may be that there are sentences hidden among other portions of the book less directly bearing on his subject which would indicate great knowledge, for although I have spent many days among his six volumes, I have certainly omitted some parts which seemed to me out of the range of stœchiometry. But his doctrines are not to be got in fragmentary sentences, so that the loss of any such sentences cannot, in the least, affect the result.

RICHTER'S STŒCHIOMETRY, VOL. I., PAGE 121.

DEFINITION 1.

"Stœchiometry (stœchyometria) is the science of measuring the quantitative proportions, or the proportions of the masses in which chemical elements stand in regard to each other. The mere knowledge of these relations might be called 'quantitative stœchiology.'

PRINCIPLE 1. P. 123.

"Every infinitely small particle of the mass of an element has an infinitely small part of the chemical attractive force or affinity.

EXPERIENCE 5.

"In order to make a neutral compound out of two elements, it is needful, as each of the elements is of the same constitution at one time as at another, to take the same quantity for the first part formed as for the second part. For example, if

two parts of lime require five parts of muriatic acid for solution, six parts of lime will require fifteen of the same acid.

EXPERIENCE 6. P. 124.

"When two neutral solutions are mixed, and a decomposition follows, the new resulting products are almost without exception neutral also, but if the solutions of one or both are not neutral before mixing, the products after mixture are also not neutral.

COROLLARY 1.

"The elements must therefore have amongst themselves a certain fixed proportion of mass. To determine which, their neutral compounds generally give the best opportunity.*

COROLLARY 2.

"If the weights of the masses of two neutral compounds which decompose each other are A and B, and the mass of the one element in A is *a*, and that of the one in B is *b*, then the masses of the elements in A are A—*a*, *a* and those in B are B—*b*, *b*. The proportions of the masses of the elements in the neutral compounds before decomposition are A—*a*: *a* and B—*b*: *b*; but after decomposition the new products are *a*+B—*b*, and *b*+A—*a*, and the proportion of the masses of the elements is *a*: B—*b*, *b*: A—*a*. If the proportion of the masses in the compounds A and B is known, that in the new products is known also.

* In German "Der Stoff ihrer neutraler Verbindungen öfters einen Bestimmungsgrund abgeben kann." Stoff is explained thus: Einleiting: Erklärung 14. "Das materielle oder körperliche Subjekt, worinnen sich die chymische Verwandtschaft befindet, nenne ich die *Masse*, *Prinzip* oder *Stoff (Massa)* des Elementes. Die Summa der Massen der Elemente, so eine neutrale auflösung bilden, ist die *Masse* oder *Stoff (Massa)* der neutralen Auflösung."

That is, "I call the material or corporal *subjectum* in which the chemical affinity resides, the Mass, Principle or Stuff (Massa)."

In a note, he says,—"There is present in the Element a certain *subjectum* to which the chemical attractive power or the affinity is bound, this is the Mass of the Element."

If $a+B-b=C$ and $b+A-a=D$ then $a=C+b-B=b+A-D$ and $C-B=A-D$, so also $D-B=A-C$. In addition $b=a+B-C=D-A+a$.

THEOREM 1. P. 125.

"The chemically attracting power by which one element *a* enters into neutrality with another A—*a* presupposes an opposite action of the same kind in the latter, and these two powers are equal to each other.

THEOREM 2. P. 128.

"If a neutral compound A whose elementary masses A—*a* and *a* are removed from combination by a definite quantity of a third element *b*, and the whole mass of one element *a* for example is set free, the force that causes this phenomenon is equal to the difference between the separating element *b* and the separated element *a*.

THEOREM 3. P. 130.

"When two neutral compounds A and B, the masses of whose ingredients are A—*a*, *a* and B—*b*, *b* mutually decompose each other, so that the new products A—*a*+*b* and B—*b*+*a* are formed, the forces that partly cause and partly hinder this action, are equal to the difference of affinities of the elements A—*a* and B—*b* towards each of the elements *a* and *b*." Afterwards he said,

"When I finished the pure stœchiometry, two years ago, I did not think it would be needful to make any additions to its contents. In the first place I thought it had all that practical stœchiometry required, &c.*

VOL. II., PAR. V., P. 4.

Having decided by experiment that 1000 parts carbonate of lime contain 559 earthy matter, he sums up as follows

* Preface to Vol. I., Part 2. 1794. Later than Vols. II. and III.

his experiment with 5760 grs. of muriatic acid and 2393 grs. of chalk.

"If now we desire to find the proportion of the elements in the pure salt forming a neutral body, we must first seek to determine the amount of lime out of the weight of the crude lime used or the aerial salt of lime. This amounts to 2393 grains. According, then, to par. 1, 1000:559 = 2393 : lime, and the lime is equal to $\frac{2393 \cdot 559}{1000} = 1337$; this, when subtracted from the 2544 grains of the neutral mass obtained, leaves a residue of 1207 grains, the weight of the muriatic acid. If, then, 1207 : 2544* = 1000 : 1107, it is clear that in the salt of lime (chloride of calcium) 1000 parts of muriatic acid are united in a neutral state with 1107 parts of lime; the proportion of the elements in this neutral solution is then best designated by 1000 : 1107.

In this manner all the earths are treated, after which he gives the relation of the quantities of alkaline earths towards sulphuric acid and each other.

Order of the masses of alkaline earths towards muriatic acid. § XXII. P. 27.

"If we set in a row the numbers which have been found representing the masses of alkaline earths which unite with 1000 parts of muriatic acid, we obtain the first series of quantities of the alkaline earths. The muriatic acid is the determining element (elementum determinans) of this series, and every member of this series represents an element determined (elementum determinatum). In order to designate the elements to which we affix these numbers, we shall make use of the chemical signs for the sake of convenience, setting the determining element, or rather its sign, at the top, or at the side of the series of quantities; and when no number is placed, we shall suppose it to be 1000. In order to fix these signs in our memory, we shall here repeat them." (These signs it is not convenient to use.)

* This evidently ought to have been 1337.

"According to the paragraphs quoted, the following is the series of the alkaline earths in their relation to muriatic acid:—

MURIATIC ACID.

ALUMINA.	MAGNESIA.	LIME.	BARYTA.
734	858	1107	3099

"Little as the members of this series appear to follow any certain order, it is nevertheless decidedly the case; at the same time the inquiry into the law of these series is one of the most difficult problems which stœchiometry gives us to solve, and if we do not go to the inquiry with sufficient practical and theoretical exactness, we shall not succeed in our inquiry into these laws or orders (of the numbers). And now, to inquire into the law of the series before us, let us first seek the difference between each member and its successor, and we obtain 858—734=124, 1107—858=249, 3099—1107=1992. Let us then use the first difference to divide the two following differences, and we obtain $\frac{249}{124}=2+\frac{1}{124}$, $\frac{1992}{124}=16\frac{8}{124}$. Then let us see if one quotient allows of division by the other, that is, let us divide $16+\frac{8}{124}$ by $2+\frac{1}{124}$. If we bring divisor and dividend under the same denomination of 124, this will be $16+\frac{8}{124}=\frac{1992}{124}$ and $2+\frac{1}{124}=\frac{249}{124}$; therefore $\frac{1992}{124}:\frac{249}{124}=\frac{1992}{249}$ and 249 is contained exactly 8 times in 1992, consequently $\frac{1992}{249}=8$. From this it is clear, that when we have the first difference $124\frac{1}{2}=\frac{249}{2}$, all the succeeding differences may be so divided by it that nothing remains; the half here mentioned is only $\frac{1}{1716}$ in 858 parts=0.0006 and still less in the other members of the series, it is, therefore, of no importance; it is impossible in experiments to arrive at such minuteness, at the same time in calculating the proportion to 1000 parts, it was necessary to throw away small unimportant fractions, otherwise it would be needful to use an enormous number of figures in order to designate the quantities. Now $\frac{249}{2}\times 2=249$; and $\frac{249}{2}\times 8=1992$, consequently $734+\frac{249}{2}=858\frac{1}{2}$, $734+\frac{249}{2}+\frac{249}{2}\times 2=1107\frac{1}{2}$; 734

$+\frac{249}{2}+\frac{249}{2}\times 2+\frac{249}{2}\times 16=3099\frac{1}{2}$. In order better to understand all, let us make $734=a$, $\frac{249}{2}=b$, then $734=a$, $858\frac{1}{2}=a+b$, $1107\frac{1}{2}=a+b+2b=a+3b$, $3099\frac{1}{2}=a+b+2b+16b=a+19b$. From this the quantitative series appears in the following order:—

MURIATIC ACID.

ALUMINA.	MAGNESIA.	LIME.	BARYTA.
a	$a+b$	$a+3b$	$a+19b$

"Now this series remains always the same, even when we put a higher or a lower number for the mass of the determining element, for if the mass of the determining element is n times greater or n times smaller, then, in the first case, all the terms would be n times greater; that is, multiplied by n; and in the latter case n times smaller or divided by n, and the order of the differences would remain always the same; because what occurs with one of the differences must occur also with the others, if otherwise the determining element must still be considered as such. When this series is attentively considered, we observe that the difference of the successive terms is a mathematical product of the first difference b with an odd number. According to it the quantities in which the hitherto known alkaline earths assert their neutrality with muriatic acid are terms of a real arithmetical progression, the terms of which are found, when the product of a certain quantity with an odd number is added to the first term, only that between them many odd numbers, such as 5, 7, 9, 11, 13, 17, are left out. This is more remarkable, as the differences which the first term makes with the succeeding ones may be represented entirely by odd numbers; for one need only suppose that the mass of the determining element is divided by b, then all the terms of the series would be at once divided by b, and appear in the following form:—

$$\frac{\text{Muriatic Acid}}{b}=\frac{1000}{249:2}=\frac{2000}{249}=8\frac{8}{249}$$

ALUMINA.	MAGNESIA.	LIME.	* * *	BARYTA. . . . .
$\frac{a}{b}$	$\frac{a}{b}+1$	$\frac{a}{b}+3$	* * *	$\frac{a}{b}+19$ . . . .

"In this case the first term would be $\frac{a}{b}$ and if it were all expressed in numbers then would $\frac{a}{b} = \frac{739}{249:2} = \frac{1478}{249} = 5 + \frac{233}{249}$ and the mass of the determining element $\frac{1000}{b} = \frac{1000}{249:2} = \frac{2000}{249} = 8\frac{8}{249}$. In this way all the members are obtained in numbers, when 1, 3, and 19 are added to the first term $\frac{1478}{249}$, and the elements observed which are designated by these figures. It is very probable that the terms $\frac{a}{b}+5$, $\frac{a}{b}+7$, $\frac{a}{b}+9$, $\frac{a}{b}+11$, $\frac{a}{b}+13$, $\frac{a}{b}+15$, $\frac{a}{b}+17$ are wanting in the series, and the reasons for considering this probable, will be shewn in a suitable place.

"*Preliminary determination of the order of the alkaline earths which enter into neutrality with vitriolic acid.* § XXIII., p. 33.

"If we put in order the amount (mass) of alkaline earths which stand in neutrality with 1000 parts of vitriolic acid, in the manner adopted with muriatic acid, the following series of quantities is obtained:—

VITRIOLIC ACID.

MAGNESIA.	LIME.	ALUMINA.	BARYTA.
616	796	1053	2226

"In order to discover the law of this series let us, as in the former case, subtract the first term from all the succeeding, and we receive 796—616=180; 1053—616=437, 2226—616 =1610. Let us see now if the first difference can so divide all the rest, that nothing, or at least very little, remains, then $\frac{437}{180}=2+\frac{77}{180}$, $\frac{1610}{180}=8+\frac{170}{180}$. As nothing can be discovered here on account of the variety in the remaining fractions, let us divide every difference by 90 as the half of the first difference, and we obtain $\frac{180}{90}=2$, $\frac{437}{90}=5$, $\frac{1610}{90}=18-\frac{10}{90}$. The fractions here are not so considerable as before, although too large to be thrown away. Until, therefore, we are able to complete the order let us make 616=616, 796=616—2.90,* 1053=616+5.90—$\frac{13}{90}$, 2226=616+18.90—$\frac{10}{90}$."

* 2.90 means 2 × 90.

"*Nearer determination of the law by which the quantities of the alkaline earths, which enter into rest and neutrality with muriatic and vitriolic acid, increase or diminish in arithmetical progression.* § XXIV., p. 34.

"A. As we cannot completely ascertain the law by which the terms of the two series of numbers obtained by experiment proceed, we must try another source of information, to the obtaining of which the series itself which the determining element of muriatic acid makes with the alkaline earths, gives us an opportunity. As the differences of the quantities in § XXII. are a product of a quantity b with an odd number, it is possible that as many terms are wanting as there are odd numbers between 3 and 19, and even that other terms may lie beyond the term $a+19b$ or $\frac{a}{b}+19$. Suppose, then, that this series were complete, namely, a, $a+b$, $a+3b$, $a+5b$, $a+7b$, $a+9b$, $a+11b$, $a+13b$, $a+15b$, $a+17b$, $a+19b$, $a+21b$, $a+23b$, &c., the masses of the elements which enter into neutrality with 1000 parts of muriatic acid would be the following:—

a			=	734			=	734
a	+	b	=	734	+	124½	=	858½
a	+	$3b$	=	734	+	3.124½	=	1107½
a	+	$5b$	=	734	+	5.124½	=	1356½
a	+	$7b$	=	734	+	7.124½	=	1605½
a	+	$9b$	=	734	+	9.124½	=	1854½
a	+	$11b$	=	734	+	11.124½	=	2103½
a	+	$13b$	=	734	+	13.124½	=	2352½
a	+	$15b$	=	734	+	15.124½	=	2601½
a	+	$17b$	=	734	+	17.124½	=	2850½
a	+	$19b$	=	734	+	19.124½	=	3099½
a	+	$21b$	=	734	+	21.124½	=	3348½
a	+	$23b$	=	734	+	23.124½	=	3597½
		&c.		&c.		&c.		

"Now let us suppose that these alkaline earths, which are partly real, partly possible, and designated by the above numbers, entered with muriatic acid into such neutral combinations as would decompose with a neutral compound out of the series § XXIII., such as the magnesia salt, by double affinity either positive or negative (see Pure Stœchiometry. Theor. 3, coroll. 3. Introd. definition 16), then only that neutral compound is excepted which the muriatic acid makes with the alkaline element of the neutral salt which we have chosen in the series XXIII., or the combination which is also taken with the magnesia salt. But according to experiment 6, coroll. 2, in the Pure Stœchiometry, in the decomposition by double affinity, out of three proportions the fourth may be determined. Let us suppose, then, that all the mentioned actual and possible neutral combinations, magnesia salt excepted, decompose with sulphate of magnesia (bitter salt) either positively or negatively, so that each constituent is placed in a state of rest (Pure Stœch. Theor. 1, coroll. 1); then we may find how many measures of each of the real and possible elements are wanted for 1000 parts of the vitriolic acid measures. (Pure Stoech. Introd. def. 14.)

"B. The first neutral compound in the series, § XXII., is an actual one, namely, alum salt, where 734 parts of alumina stand in neutrality with 1000 parts of muriatic acid. If this neutral or middle salt decomposes with sulphate of magnesia by double affinity, then 858½ parts of magnesia must be contained in the sulphate of magnesia, because the proportionate quantity in the magnesia salt is as 1000 : 858, or rather 1000 : 858½. § XXII. Now the proportionate quantity in the sulphate of magnesia is 1000 : 616 (§ XIX., XXIII.) and 616 : 1000=858½ : 1,394; that is, if 616 parts of magnesia insist on rest with 1000 parts of vitriolic acid, the same must occur between 858½ parts of the first and 1394 parts of the latter; therefore when alumina salt and magnesia salt decompose, 1000 parts of muriatic acid dissolve with 858½

parts of magnesia, and 1394 parts of vitriolic acid with 734 parts of alumina; the proportionate amount of the alum formed will then be 1394 : 734=1000 : 526, which is not the proportion of the neutral but the common alum. (§ XXI.) The quantity of the alkaline earth in common alum belongs accordingly to the series § XXIII.

" C. But when, in the decomposition of the first neutral compound, where muriatic acid is the determining element with sulphate of magnesia, the amount of the vitriolic acid is 1394, it is so in all subsequent possible decompositions which the neutral compounds of the other actual and possible elements of the series § XXII. make with the sulphate of magnesia, let these decompositions be positive or negative. (Pure Stœch. Theor. 3, coroll. 3.) The quantity, then, of real and possible elements which belong to 1000 parts of muriatic acid belong to 1394 of vitriolic, and the following proportions are obtained for the compounds where the vitriolic acid becomes at rest with the real and possible elements (Pure Stoech. Theor. 1, coroll. 1), all of which obtained by experiment except the common alum are neutral.

1394	:	734	=	1000	:	526
1394	:	858½	=	1000	:	616
1394	:	1107½	=	1000	:	796
1394	:	1356½	=	1000	:	973
1394	:	1605½	=	1000	:	1152
1394	:	1854½	=	1000	:	1330
1394	:	2103½	=	1000	:	1508
1394	:	2352½	=	1000	:	1687
1394	:	2601½	=	1000	:	1866
1394	:	2850½	=	1000	:	2045
1394	:	3099½	=	1000	:	2224
1394	:	3348½	=	1000	:	2402
1394	:	3597½	=	1000	:	2580

§ XXV.

" A. When we look on all these numbers found, viz., 526, 616, 796, 973, &c., as quantities of the elements which are at rest with 1000 parts of sulphuric acid, we obtain a series, the law of which soon appears to us. Let us first subtract the first term from all the succeeding, and we obtain the following differences, which may be expressed in various ways :—

616	—	526	=	90	=	90			=	90		
796	—	526	=	270	=	270			=	3.90		
973	—	526	=	447	=	450	—	3	=	5.90	—	3
1152	—	526	=	626	=	630	—	4	=	7.90	—	4
1330	—	526	=	804	=	810	—	6	=	9.90	—	6
1508	—	526	=	982	=	990	—	8	=	11.90	—	8
1687	—	526	=	1161	=	1170	—	9	=	13.90	—	9
1866	—	526	=	1340	=	1350	—	10	=	15.90	—	10
2045	—	526	=	1519	=	1530	—	11	=	17.90	—	11
2224	—	526	=	1698	=	1710	—	12	=	19.90	—	12
2402	—	526	=	1876	=	1890	—	14	=	21.90	—	14
2580	—	526	=	2054	=	2070	—	16	=	23.90	—	16

" B. The law by which the differences of the actual and possible alkaline earths increase in relation to vitriolic acid is then so far made out that it follows from the product of a number which is here 90 with the consecutive odd numbers; we may consider the numbers which are to be subtracted, such as 3, 4, 6, 8, 9, &c., as nothing, because the greatest error that could be caused is only $\frac{3}{450}=\frac{1}{150}=0.0066$, or $\frac{66}{10000}$; but even this is not necessary, because the numbers themselves proceed in distinct order, as the series shews; and if we inquire, as in § XXII., into the manner in which these numbers progress, we observe that if three of them increase

by odd numbers, the succeeding four increase in the ordinary way by one, and so alternately; for example.

—	3			=	—	3
—	(3	+	1)	=	—	4
—	(3	+	3)	=	—	6
—	(3	+	5)	=	—	8
—	(3	+	6)	=	—	9
—	(3	+	7)	=	—	10
—	(3	+	8)	=	—	11
—	(3	+	9)	=	—	12
—	(3	+	11)	=	—	14
—	(3	+	13)	=	—	16
—	(3	+	15)	=	—	18
—	(3	+	16)	=	—	19
		&c.		&c.		

"These quantities accordingly proceed in arithmetical progression down to the most insignificant fractions, as we may see by a glance at them.

"*The quantities in which the alkaline earths enter into neutrality with muriatic acid are terms of an endless series, which increase by the product of a determinate quantity with the consecutive odd numbers. The same thing occurs with the alkaline earths in relation to sulphuric acid, only that in this case a quantity must be taken from the terms of the last series, the first three excepted; this quantity increasing also in progression.*

§ XXVI.

"A. After finding out the law by which the quantities of the alkaline earths increase towards the two acids (sulphuric and muriatic), it becomes necessary to form the series themselves, that we may see clearly the correctness of the proposition advanced as a hypothesis; for if it is done rightly the proposition ceases to be a hypothesis. We shall designate the terms that are wanting in both series by a star, and the

elements which produce with acids a very violent heat when freed from their air, for example, lime and magnesia, by △, as the sign of fire.

Muriatic Acid .... $a = 734$, $b = \frac{249}{2} = 124\frac{1}{2}$.

	Alumina...	a			=	734	+		=	734
△	Magnesia..	a	+	b	=	734	+	$\frac{249}{2}$	=	$858\frac{1}{2}$
△	Lime	a	+	$3b$	=	734	+	$\frac{3 \cdot 249}{2}$	=	$1107\frac{1}{2}$
	*	a	+	$5b$	=	734	+	$\frac{5 \cdot 249}{2}$	=	$1356\frac{1}{2}$
	*	a	+	$7b$	=	734	+	$\frac{7 \cdot 249}{2}$	=	$1605\frac{1}{2}$
	*	a	+	$9b$	=	734	+	$\frac{9 \cdot 249}{2}$	=	$1854\frac{1}{2}$
	*	a	+	$11b$	=	734	+	$\frac{11 \cdot 249}{2}$	=	$2103\frac{1}{2}$
	*	a	+	$13b$	=	734	+	$\frac{13 \cdot 249}{2}$	=	$2352\frac{1}{2}$
	*	a	+	$15b$	=	734	+	$\frac{15 \cdot 249}{2}$	=	$2601\frac{1}{2}$
	*	a	+	$17b$	=	734	+	$\frac{17 \cdot 249}{2}$	=	$2850\frac{1}{2}$
	Baryta ..	a	+	$19b$	=	734	+	$\frac{19 \cdot 249}{2}$	=	$3099\frac{1}{2}$
	*	a	+	$21b$	=	734	+	$\frac{21 \cdot 249}{2}$	=	$3348\frac{1}{2}$
	*	a	+	$23b$	=	734	+	$\frac{23 \cdot 249}{2}$	=	$3597\frac{1}{2}$
				&c.				&c.		

" B. Before we set down the quantitative progression in the case of vitriolic acid, we must first inquire if the quantity of alumina in neutral alum belongs to this series; it is 1053. Let us subtract 526 from 1053, and we obtain 527; now 527 = 540—13=6.90—13, and consequently 1053=526+6.90—13. But as the series determined by vitriolic acid proceeds by the uninterrupted odd numbers, and no neutral alum can be found in decomposing by double affinity, the quantity 526+6.90—13 does not belong to this series. We must take it, however, in the meantime into the series, because it belongs to the quantities which enter into neutrality. We shall, however, put such in brackets, as must happen when considering the quantity of alumina in common alum, if it is not a legitimate member of the series, and capable of double affinity.

No. 2.

Sulphuric Acid......$a = 526,\ b = 90$

	Alumina.. a	$=526$	$= 526$
△	Magnesia. $a+b$	$=526+90$	$= 616$
△	Lime ... $a+3b$	$=526+3.90$	$= 796$
*	$a+5b-3$	$=526+5.90-3$	$= 973$
	Alumina..$(a+6b-13)$	$=526+(6.90-13)$	$=1053$
*	$a+7b-(3+1)$	$=526+7.90-(3+1)$	$=1152$
*	$a+9b-(3+3)$	$=526+9.90-(3+3)$	$=1330$
*	$a+11b-(3+5)$	$=526+11.90-(3+5)$	$=1508$
*	$a+13b-(3+6)$	$=526+13.90-(3+6)$	$=1687$
*	$a+15b-(3+7)$	$=526+15.90-(3+7)$	$=1866$
*	$a+17b-(3+8)$	$=526+17.90-(3+8)$	$=2045$
	Baryta .. $a+19b-(3+9)$	$=526+19.90-(3+9)$	$=2224$
*	$a+21b-(3+11)$	$=526+21.90-(3+11)$	$=2402$
*	$a+23b-(3+13)$	$=526+23.90-(3+13)$	$=2580$
	&c.	&c.	

"C. If we convert the differences into simple consecutive odd numbers, the quantity of the determining element and all the terms of the series have only to be divided by b, and we receive:

No. 1.

$\frac{\text{Muriatic Acid,}}{b}\quad a=734,\ b=\frac{249}{2}\quad \frac{\text{Muriatic Acid,}}{b}=8\frac{+8}{249}$

	Alumina.. $\frac{a}{b}$	$=\frac{734}{124\frac{1}{2}}$	$=5+\frac{223}{249}$
△	Magnesia. $\frac{a}{b}+1$	$=\frac{858\frac{1}{2}}{124\frac{1}{2}}$	$=6+\frac{223}{249}$
△	Lime.... $\frac{a}{b}+3$	$=\frac{1107\frac{1}{2}}{124\frac{1}{2}}$	$=8+\frac{223}{249}$
*	$\frac{a}{b}+5$	$=\frac{1356\frac{1}{2}}{124\frac{1}{2}}$	$=10+\frac{223}{249}$
*	$\frac{a}{b}+7$	$=\frac{1605\frac{1}{2}}{124\frac{1}{2}}$	$=12+\frac{223}{249}$
*	$\frac{a}{b}+9$	$=\frac{1854\frac{1}{2}}{124\frac{1}{2}}$	$=14+\frac{223}{249}$
*	$\frac{a}{b}+11$	$=\frac{2103\frac{1}{2}}{124\frac{1}{2}}$	$=16+\frac{223}{249}$

$$* \quad \frac{a}{b} + 13 = \frac{2352\frac{1}{2}}{124\frac{1}{2}} = 18 + \frac{223}{249}$$

$$* \quad \frac{a}{b} + 15 = \frac{2601\frac{1}{2}}{124\frac{1}{2}} = 20 + \frac{223}{249}$$

$$* \quad \frac{a}{b} + 17 = \frac{2850\frac{1}{2}}{124\frac{1}{2}} = 22 + \frac{223}{249}$$

$$\text{Baryta..} \quad \frac{a}{b} + 19 = \frac{3099\frac{1}{2}}{124\frac{1}{2}} = 24 + \frac{223}{249}$$

$$* \quad \frac{a}{b} + 21 = \frac{3348\frac{1}{2}}{124\frac{1}{2}} = 26 + \frac{223}{249}$$

$$* \quad \frac{a}{b} + 23 = \frac{3597\frac{1}{2}}{124\frac{1}{2}} = 28 + \frac{223}{249}$$

&c. &c.

No. 2.

Further—

Vitriolic Acid, $a = 526$, $b = 90$ $\quad \frac{\text{Vitriolic Acid}}{b} = 11\frac{1}{2}$

$$\text{Alumina.} \quad \frac{a}{b} = \frac{526}{90} = 5 + \frac{76}{90}$$

$$\text{Magnesia} \quad \frac{a}{b} + 1 = \frac{616}{90} = 6 + \frac{76}{90}$$

$$\text{Lime ..} \quad \frac{a}{b} + 3 = \frac{796}{90} = 8 + \frac{76}{90}$$

$$* \quad \frac{a}{b} + 5 - \frac{3}{b} = \frac{973}{90} = 10 + \frac{73}{90}$$

$$\text{Alumina.} \quad \left[\frac{a}{b} + 6 - \frac{13}{b}\right] = \left[\frac{1053}{90}\right] = \left[11 + \frac{63}{90}\right]$$

$$* \quad \frac{a}{b} + 7 - \frac{(3+1)}{b} = \frac{1152}{90} = 12 + \frac{72}{90}$$

$$* \quad \frac{a}{b} + 9 - \frac{(3+3)}{b} = \frac{1330}{90} = 14 + \frac{70}{90}$$

$$* \quad \frac{a}{b} + 11 - \frac{(3+5)}{b} = \frac{1508}{90} = 16 + \frac{68}{90}$$

$$* \quad \frac{a}{b} + 13 - \frac{(3+6)}{b} = \frac{1687}{90} = 18 + \frac{67}{90}$$

$$* \quad \frac{a}{b} + 15 - \frac{(3+7)}{b} = \frac{1866}{90} = 20 + \frac{66}{90}$$

$$* \quad \frac{a}{b} + 17 - \frac{(3+8)}{b} = \frac{2045}{90} = 22 + \frac{65}{90}$$

$$\text{Baryta..} \quad \frac{a}{b} + 19 - \frac{(3+9)}{b} = \frac{2224}{90} = 24 + \frac{64}{90}$$

$$* \quad \frac{a}{b} + 21 - \frac{(3+11)}{b} = \frac{2402}{90} = 26 + \frac{62}{90}$$

$$* \quad \frac{a}{b} + 23 - \frac{(3+13)}{b} = \frac{2580}{90} = 28 + \frac{60}{90}$$

&c. &c.

" D. When the numbers in the last series are compared with those found by experiment, they are found to agree perfectly, as far as regards alumina, lime, and magnesia. On the other hand, the amount of baryta in the sulphate is 2224, but in § XIX. 2226. No doubt the difference comes from the greater difficulty in finding the point of saturation in the case of this earth than in the case of the others, when combining them with sulphuric acid. At the same time, the supposed error is so small that it may be left out of consideration, for it amounts only to $\frac{2}{2224}$ or 0.0009 or $\frac{9}{10000}$, which is a difference that may be reckoned as nothing. It must be remembered that decimal fractions only are used here.

" E. Now if the quantities found by experiment exactly fit into the series, if all the terms of these series entirely correspond to the possibility of double affinity, if even a quantity which is capable of neutrality, but not of double affinity, is banished out of one series by the rule of the series; further, if one series becomes possible only through the other, the proposition is absolutely certain, that the quantities of the hitherto known alkaline earths which enter into rest or equilibrium with sulphuric and muriatic acids are terms of an infinite series in arithmetical progression, each of which proceeds according to its own law.

" G. Shall we then conclude from the laws of the two series, in which so many terms are wanting, that there are many alkaline earths existing in nature? So far as probability and possibility are concerned it is a fair conclusion, especially since the knowledge of magnesia and baryta are the property of only the last half century. If they had not been discovered until after the study of the stœchiometric sphere had commenced, the second and eleventh term of every series would be wanting, and a * substituted, and besides it would not have been possible, with such a small number of terms to have found out the law. Who knows if there are not other elements in existence which interrupt the last series as neutral quantities, precisely as with

the alumina? But if we should conclude from the law of these series that the existence of the failing elements is necessary, then we should commit as great an error as if we were to conclude that a planet must exist between Mars and Jupiter, because it corresponds to the law of the distance of the planets from the sun.

"H. The use of these series of quantities is not small, for if we know only the first member and are acquainted with the law, we find all the other members and all the proportionate quantities with the greatest exactness, and who does not know what differences there have hitherto been in the numbers representing the proportions? How many uses shall we find also in chemical analysis for series of this kind, of which probably there are many, and to what perfection might it not bring the chemical system, if they could be used as tables of affinities?

"*The two series of quantities,* 1 *and* 2, § XXVI., *are really quantitative series of the affinity of alkaline earths towards muriatic and sulphuric acid.* Page 51.

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Page 56. "*Determination of the decomposing forces,* § XXVIII.

"A. The experiments now detailed enable us to state the proposition that affinities are as the masses.

"C. P. 61. If now in these cases of affinity the attracting forces of the elements are as the masses of the elements, and we take 3099 as the attracting force or affinity of baryta towards muriatic acid, the numbers 1107, 858, 734, or the attractive force of the elements represented is quite unaltered towards muriatic acid, on the other hand we must calculate the affinity of these earths to sulphuric acid from the numbers given and the proposition adopted. According to the proposition, 1000 : 1394=3099 : 4320 and 1000 : 1394=734 : 1023, in the same way 3099 : 734= 4320 : 1023. If, then, the attractive power of baryta towards muriatic acid is 3099, towards

vitriolic acid it is 4320, and if alumina acts towards muriatic acid with the force of 734, and towards vitriolic acid with a force of 1023 forming common alum, in the same manner the baryta is attracted with a force of 4320 towards vitriolic acid, forming heavy spar, and so the power by which this acid forms common alum is only 1023. If we inquire into the affinity of the other alkaline earths towards vitriolic acid by the rule of three, we obtain for lime $\frac{1394 \cdot 1107}{1000}=1543$, for magnesia $\frac{1394 \cdot 858}{1000}=1196$. If, then, in the cases of double affinity given, we put instead of the quantity the attractive force by which one element works on another, we obtain, according to the first theorem of the 'Pure Stœchiometry,' as follows:—

No. 1.

Baryta.		Muriatic Acid.
3099	Baryta Salt.	3099
4320		734
	Heavy Salt.	
	Alum. Spar.	
734		4320
1023	Common Alum.	1023
Alumina.		Vitriolic Acid.

"D. * * * In No. 1 the two positive or decomposing elements are 4320 and 734, the negative which hinder the compound are 3099 and 1023, consequently 4320+734=5054 the whole positive or furthering, and 3099+1023 =4122 the whole negative or hindering power. * * *" The difference (equal to the power, as he says) =+932 is positive.

He then endeavours to shew in the same way, page 171, &c., that "The masses (quantities) of the three alkaline salts,

which enter into neutrality with an equal amount of vitriolic or muriatic acid, are the three first terms of two series, of which, that which belongs to muriatic acid proceeds by the odd numbers without interruption, and the other is the product of a quantity with the numbers in regular succession."

Page 167. "When an aqueous solution of vitriolic salammoniac is poured into a solution of muriate of lime, an abundant precipitate is caused, which is completely formed gypsum; if the exact quantity of the salammoniac solution has been used which is necessary to complete the precipitation, the liquid above the precipitate contains nothing but perfectly formed common salammoniac. But the proportion in the salt is 1000 : 1107½, and to 1000 of the chloride are to be calculated 889 parts of the volatile alkali; now let us inquire how much of the vitriol is needed for 1107½ parts of the lime, the proportion of the last to the first is 796 : 1000, consequently 1107½ parts of lime demand $\frac{1000 \cdot 1107\frac{1}{2}}{796}=\frac{2215000}{1592}=1394$ parts of vitriol, which belong to the 889 parts of the volatile alkali.

He gives a list, page 279, of "*Proportional quantities of neutral compounds which decompose each other, when entirely deprived of water.*" * * * * "In each of these cases it is only necessary to add the numbers representing the quantities standing against each other horizontally, by which the power of the affinity is estimated, and we receive the neutral quantities which decompose each other, and consequently their proportion."

Salammoniac Vitriol.		Common Salt.
689 + 1000	:	960 + 717 = 1689 : 1677
Salammoniac Vitriol.		**Common Salt.**
638 + 1000	:	960 + 717 = 1638 : 1677
Magnesia Salt.		**Vitriolized Potash.**
858 + 1000	:	2239 + 1394 = 1858 : 3633

&c., &c., &c.; this is from a list of 28: the second and third are supposed hydrous.

Then we have, p. 284, "*Proportional quantities of neutral compounds containing muriatic acid, considered as anhydrous, when decomposed by vitriolic acid.*" Also, "*Proportional quantities, when the neutral compounds which vitriolic acid makes with the alkaline salts and magnesia are decomposed negatively or by free muriatic acid.*" P. 293.

At page 190, he says, that the affinities are as the amount of the combining proportions, and here also the atomic weights of ammonia, soda, and potash, are such as to lend some countenance to it. The series, however, is still considered the most important thing, and he finds afterwards, in the vol. for 1800, that smaller weights may precipitate larger ones.

These inquiries were continued with great labour, and in his work "On the newer subjects in chemistry," we have many attempts to define the relations between the acids and bases. In the vol. for 1798, we find him fixing the relation between the metals and some of the acids, but always on the same plan.

At page xv. in the preface to the vol. for 1800, he says, "To follow an author step by step, in a path trodden by him alone, and to judge him with fairness, is not in the power of every one, still less can it be done by merely reading through his book."

Page xxiii. Again, "Whoever looks on the remarkable order, which reigns in the quantitative proportions, by which every kind of substance has a peculiar quantitative character with respect to another, as a mere play of figures, or as a mere accident, would only show his complete ignorance of the whole structure of stœchiometry, but would be indemnified for it by a still greater degree of philosophical faith; for it requires much more credulity to believe in so many accidents, than is needed to perceive that the Lord of nature has not only qualitatively but quantitatively endued it with the most wonderful order, both in great things and in small."

Another extract from the same, page 206, "In the simpler

affinities every kind of neutralizable substance has its own quantitative law of affinity, because the amount of affinities among the alkalies may be expressed by the mass, that of the acids by the substratum (that is, the body of which the oxygen of the acid is an oxide); but this is not found to be the case either with the metallic or nonmetallic combustible elements."

What then did Richter attain to is the question to be now answered. In the extract from the preface he raises the study of atomic chemistry to a science, and gives it a name. This is itself no small honour. The chemists before him had certainly not been gifted with such a clear appreciation of the importance of the study. We find that Richter has made it the leading object of his life to elucidate the laws of combination; as a young beginner, making it a subject of his inaugural dissertation, and looking forward to the time when he might have opportunity to prosecute his investigations. The word stœchiometry is preserved in Germany, with us it is too abstract for daily use.

The first definition of stœchiometry has appended to it six *experiences* (erfahrung), most of them with corollaries (zusatz). The reading becomes, therefore, exceedingly cumbrous, the words are marvellously multiplied, pure abstraction is aimed at in every step with painful strains, as it would appear, or perhaps only caused by a mathematical habit of mind too exclusively followed. In this way the few truths that we still hold to, and which are contained in the book, are so ornamented and overdressed as to have been to most persons entirely hidden under the richness of the elaboration.

He expresses his belief that the smallest portions of a body are of the same composition as the largest. He says that the affinity exists in every particle. Then adds that every piece must have the same composition. His own words are very cumbrous, but this meaning is distinctly there. This was the illustration which Dalton afterwards used on the same

subject, but it was expressed in clearer words, and still earlier, by Higgins. This idea leads directly to the atomic theory and theory of equivalents. Here it is not followed out.

The sixth *experience* of first definition gives the theory of reciprocal saturation, when double decomposition takes place in solutions. This is the discovery which has been attributed to Wenzel. Let us translate his formulas into the present symbols by an example:—

$$AgO\ NO_5—NO_5,\ NO_5+KO\ SO_3—SO_3,\ SO_3$$
$$=NO_5+KO\ SO_3—SO_3+SO_3+AgO\ NO_5—NO_5.$$

He says the products of neutral salts are nearly without exception neutral, but nevertheless sees enough to form a law. Wenzel, with similar results, had not seen a law.

He endeavours to shew the relative amount of force exerted by different substances when decomposition takes place, but he gets no farther than the fact that certain forces are equal, some must be greater, and others must be less. In this district of inquiry, an example of which may be found in Theorem I., what appears to be the enunciation of an important law, frequently turns out to be the mere expression of a common-place, giving no information to the chemist. Such laws being in a certain sense universal, they are now left out of chemical works, as the mind can readily draw the conclusion for itself, if the opportunity offers.

He then shews the method of obtaining the proportion of the elements in a compound. This had been pursued with great care by Wenzel.

The great aim of Richter is not perceived in reciprocal proportion, but in the attempt to make the combining numbers of all bodies a series in arithmetical progression, and so to bring number, quantity, and order into the arrangement of the elements. In the series which he has formed, I think we may say that he has failed to prove his point. The numbers he had were too few, and the mode of obtaining the order is by no means satisfactory. There is, however, a

great probability recognised by most chemists of the existence of an order in which the elements are related to each other. If this order should ever be found to be similar to that which Richter has indicated, we must do the greatest honour to his genius, although we cannot even now, when it stands before us, say that it is a discovery, or that it has any value at all.

The discovery of reciprocal proportion is given by no one before Richter as far as I know, but he himself does not speak of it as a discovery, but as a well-known fact, with which he was familiar before he wrote his inaugural dissertation. We find in the preface that it was well known that neutral salts gave neutral results on decomposition; this Richter has put formally amongst the laws of stœchiometry, and given it rank amongst chemical truths. He deduced from it, as he himself says, that there must be "distinct proportionate quantities amongst the component parts of neutral salts," and he strove hard to bring all combinations under number and quantity. The knowledge of this fact seems to have first set in motion his stœchiometry; instead then of being the point which he gained, it is the point from which he starts, according to his own account. He does not, however, seem to have seen the reason for it, nor its general bearing in chemistry, otherwise it could not have been left for Fischer to shew that the combining number of an element would fit its combination with every other element.

The mode in which he obtains the relation of the combining weights of the earths to each other is remarkably self-delusive, but at the same time exceedingly ingenious. They are given at length, so that every one may compare for himself.

He endeavours to find out a similar relation between the atomic weights of the alkalies, and readily does so. He is led away by the numbers observed to mistake them for representations of actual force, and so calculates in a relative and abstract way the force needed for decomposition. This is

very characteristic of him, but unfortunately he has gone on a wrong assumption.

He has evidently been a man of great quickness, at the same time apparently of haste; he has enunciated the most beautiful truths, and left them untouched for worthless speculations, which seemed to need more ingenuity, almost leaving us to doubt how far he understood his own writings. We must, however, give him the honour of understanding what he wrote, smaller honour we can give no man. Still it is perfectly clear that if his theory were as fully developed in his mind as we with our superior opportunity can now see to be deducible from his words, men would have understood him, and the process would have been continued, but neither did he make any advance, nor did he teach others clearly, although the young Berzelius was much excited to curiosity.

It certainly is difficult to tell how discoveries grow, often impossible to tell who is the discoverer; but this we may consider a fair rule, not always easily applied, it is to be confessed, that he is a discoverer who sees distinctly the full bearing of his discoveries; when this does not happen there is a difficulty in giving that man the place due to him. It is clear that Richter, like some others already mentioned, had fundamental principles which would have led him to the atomic theory; but he has evidently been led by foregone conclusions, and the law of planetary distances has been floating in his mind and misleading him, when seeking for the differences in the combining weights of bodies.

The discovery of reciprocating proportion was a very important and memorable one, although the scientific world did not recognise it, another among the many proofs that scientific men are subject to the same bigotted attachment to the laws they have learned, as that class of men hitherto most blamed for bigotry, nor is there any bigotry more engrossing than that which appears to the possessors to be upheld by experimental proof. Who discovered this important fact, it is still

left unascertained: as the expression of a law it is Richter's, but as a fact regarding neutral salts the author appears not to be known.

Among the many disputes on this point it is rather surprising that people should speak without reading the authors they discuss. The supporters of Wenzel have not read him, the supporters of Richter have overlooked his own writings and his own confession, as it appears to me in the preface. But as I give the words every one may judge for himself.

In proceeding with his inquiry one cannot but admire the energy and activity of Richter's mind, and his enthusiastic desire to prove the beauty of the arrangement of creation; it is clear that he lost his way, and spent the greatest part of his energy on a subject which could not with his data lead to great results, and which even now gives us no help, and which was not the next step wanted in chemistry. The science was straining after definite laws, it had none; Richter, with his one great law, might have done wonders, had he only seen its value; he might have found on examination that it was, properly speaking, an inference from another much more general law, and would have then expressed himself in universal terms. But Higgins had expressed himself much more clearly as to combination before him, as already shewn, and only failed because he had not seen it to be a general law.

Richter attempted to give the proportion of the acids to bases as an expression of affinity, but this had been already attempted by Kirwan, and was shewn to be unsuccessful.

As a general summary of Richter's most important work, we may say, he found that there was a certain quantitative relation between all bodies, and he made out the laws so far, that when he knew the quantitative analysis of a salt, he could tell its quantitative decomposition with another, but he never saw it with sufficient clearness to be able to express the combining quantities each by its own distinct number,

nor does he appear to have ever proceeded far enough to be able to assign a cause for the phenomenon, or to connect it with any fundamental idea.

He was the founder of the systematic study of stœchiometry, he was an illustrator of one of its important laws, and a defender of regularity in nature. His scientific life was laborious, his love of science sincere, and in all respects he seems to have been a man of high character. After reading his works, and coming occasionally on a sentence which makes us for the moment believe that he has discovered a greater law than we can give to him, and finding that during his whole life he was just on the point of the present atomic laws, one feels that he was perhaps the only man that deserved to discover them, having given himself up entirely to that purpose. It is with regret, therefore, that I leave him also, another combatant who died before the victory.

It has been said that Dalton had read Richter, and had never acknowledged his claims. It is a melancholy thing to see men of talent and learning so readily distrusting their own class, as if dishonesty were so common. I might say the same of Richter, that for more than ten years he continued to publish on stoechiometry, and never once mentioned Higgins, but his whole works shew that he did not see Higgins's writings, or he would have probably got less involved than he did. We learn from Dr. Henry that Dalton had seen Richter's results on reciprocal proportion,* and had received assistance from them, but although they may have assisted him in proving his laws, Richter could never have given him

* Dr. Thomson had said the contrary; but let us take Dr. Henry's information, as being an intimate friend. Dalton could not have seen Richter's whole works, but probably an account of them. They are scarce in England. It cost me a good deal of trouble to get one, even in Germany. The British Museum does not possess a complete copy. Dalton certainly had not read Higgins; and although Dr. William Henry had a copy, we may conclude from his son's work that he had not seen in it the Atomic Theory, as he seems not to have thought it necessary to mention its existence to Dalton.

fundamental ideas. These are much wanted in Richter's chemistry. Richter's cotemporaries did not obtain the atomic theory, although some were students of his work. Berzelius himself did not obtain the atomic theory from Richter, although the most illustrious of the students of Richter's books. Dalton then could not have obtained it, and the direction he takes is perfectly different, the road he went quite clear, and the result he came to entirely distinct from that aimed at by Richter.

In such early days it required a mind of a high order to see as Richter did into the great necessity of permanent laws, and the great structure he raised to make the inquiry shews us that he saw its importance. Had chemists been accustomed to study the works of their own class, such books as his would have rapidly produced results, but the history of the matter speaks ill for the apathy of the men of even that period, and well for his untiring energy and devotion.

CHAPTER X.

FISCHER, BERTHOLLET, PROUST, &c.

WHEN Richter had illustrated the action of neutral salts, and had seen somewhat dimly reciprocal proportion, Fischer (Ernst Gottfried) saw it in a much clearer light, and put it in a more practical form. He took Richter's analyses, and shewed that a constant quantity of one base would unite to a constant quantity of an acid, and that the numbers in all would be reciprocal. Such, one would call the true discovery of reciprocal proportion, were it not, that in this case, the intellectual labour required does not seem great.

Fischer's table is as follows:—

BASES.		ACIDS.	
Alumina	525	427	Fluoric.
Magnesia	615	577	Carbonic.
Ammonium	672	706	Sebacic.
Lime	793	712	Muriatic.
Soda	859	755	Oxalic.
Strontian	1329	979	Phosphoric.
Potash	1605	988	Formic.
Baryta	2222	1000	Sulphuric.
		1209	Succinic.
		1405	Nitric.
		1480	Acetic.
		1583	Citric.
		1694	Tartaric.*

It is explained so; "When any substance is taken from one of the two columns, *ex. gr.*, potash from the first column, where the number 1605 stands, then every number of the second column shews how much of that substance is needed to neutralize 1605 of potash." The same thing may be said

* Schweigger's *Stœchiometrische Reihen*. Page 45.

if we begin with the second column. In other words, the numbers attached to the bases indicate the amount which unites to the acid numbers (or proportions represented by them).

But we cannot say more of Fischer than that he made plainer Richter's law.

In 1803, immediately after the last of Richter's periodical publications, but ten years after the publication of *Pure Stœchiometry*, appeared the *Essai de Statique Chymique* of Berthollet.*

There is no stronger proof of the want of influence of all preceding inquirers on this subject than the existence at this period of Berthollet's essay, and the effects it had on the followers of the science, who could neither have understood nor believed the earlier, when they listened to the later with so much attention.

In Berthollet's *Essai*, we find the following sentences.

Chapter II. 13. "The chemical action of different substances is excited, not only in the ratio of their affinity, but also in the ratio of their quantity; one immediate consequence is, that chemical action diminishes in proportion as saturation advances.

14. "It also follows, from this law, that a substance which is in solution in a quantity of liquid greater than is necessary, is retained therein by a more powerful action, and that, on the contrary, the superfluous quantity of the liquid is subjected more feebly to the affinity of the dissolved substance than is required for solution. It will be seen, therefore, that the general law which I have announced is, in this instance, only modified by the circumstance which limits the quantity of liquid that can exert its action simultaneously."

Then we have this extraordinary sentence, proving that so far from the subject of the atomic theory being clear, even the doctrine of proportion had not made its way.

* The English translation by B. Lambert, 1804, is here quoted.

39. "Some chemists, influenced by having found determinate proportions, in several combinations, have frequently considered it as a general law that combinations should be formed in invariable proportions; so that, according to them, when a neutral salt acquires an excess of acid or alkali, the homogeneous substance resulting from it is a solution of the neutral salt in a portion of the free acid or alkali."

"This is a hypothesis which has no foundation, but a distinction between solution and combination."

42. "It follows, from what has been said above, that the most powerful, as well as the weakest chemical action, is exerted in the ratio of the reciprocal affinity of the substances, and of the quantities within the sphere of activity; that the action diminishes in proportion to the saturation, and that there is *no point* at which it determines the proportions, but that the limits of these proportions in the combinations which it forms, and those of its power, are to be sought for in the forces which are opposed to it. Finally, two effects of chemical action must be distinguished, that by which it produces a reciprocal saturation, and that which causes a change in the constitution."

47. "Hence it follows, that in the comparison of the acids, the first object which will fix the attention is the power with which they can exercise the acidity which forms their distinguishing character. Now this power is estimated by the quantity of each of the acids which is required to produce the same effect, that is to say, to saturate a given quantity of the same alkali."

Beautiful and ingenious as Berthollet's investigations into affinity are, he, too, missed the line of thought which was to produce the greatest discovery known to chemistry, but it was not carelessly passed over. His inquiries had led him into some very interesting qualities of bodies, the power of quantity to overpower feebler quantities in certain cases, and the capacity of bodies to decompose others according to the nature of

the compound formed. For example, a salt, say an oxalate, decomposes a salt of lime, not because the oxalic acid has such a powerful affinity for lime, but because the oxalate of lime being insoluble falls, following the physical qualities of the compound; oxalic acid too decomposes common salt, not from a greater affinity, but because muriatic acid being volatile, has a way of escape, whilst oxalic acid has not, and so on.

These and similar reasonings took up such a large portion of his field of vision, that he denied, as we have seen, definite proportions, and, of course, atomic theories and equivalents could never be received from Berthollet. He denied definite proportions in the present sense, but, as Berzelius says, allowed them, although within certain limits. This would imply proportions of a rather indefinite kind. In the "Mémoires de la Société d'Arcueil," he even gives laborious analyses tending to fix the proportions of the elements in certain bodies. This was chiefly from the point of view that distinct bodies had determinate and constant composition, which, of course, it would have been too late to deny.

Berthollet did, nevertheless, put a drag on the inquiry into the general question of proportion, and from his superior position commanded great influence. It is in this state we find the science, then, when Dalton came to it. It may be said that the state of the science comprehended all that was published, and this is in a larger sense true, and constitutes the base of a final opinion of a discoverer's merits; but the state of what seemed to be the well-grounded science, as far as the leading men held it, was rather that of Berthollet than any others, the leading innovators were obscure, and never indeed became magnates in the scientific world. These latter commanded to all appearance in the capital, whilst the real power was getting prepared in the provinces.

Berthollet's essays must always stand as the greatest proofs of the reality of Dalton's achievements. Whatever men may

have built before, this is the existing ground on which Dalton had to raise his edifice.

To some extent this may be reckoned questionable, as the discussion as to the definite proportions of compounds was going on with Proust, a strong supporter of this doctrine, but this exception only shows that the differences of opinion were raised, not on the great theory, the history of which we are discussing, but on the simplest preliminary facts; the laws themselves did not appear in the discussion from the cause already given, viz., virtual non-existence, except in a state of possible evolution from laws which themselves were not fully proved.

I shall now give those sentences from Proust's writings which seem most nearly to affect our subject.

* "As to those which have been announced by Thenard, I will not contest results obtained by a chemist who knows how to operate with that exactitude which characterizes a consummate worker. I will say, however, without attaching any importance to my opinion, that in considering this almost general law of nature which offers us everywhere only one or two terms of oxidation of metals, and from which, in our artificial imitations, we cannot free ourselves, I fear that the six terms which he recognises are not all sanctioned by nature.

"If by the assistance of a high temperature we lower the weight of an oxide which is at its maximum, and which does not happen to be volatile, or if by a high continued heat we elevate a metal to its highest oxidation, are we to believe that all the ascending or descending terms of oxidation, which may be inserted between the extremes, are to be taken as so many different terms of oxidation? Certainly not. I do not recognise in that the ordinary course of nature, and I venture to believe that in similar cases we only make mixtures in all possible proportions of the oxide at minimum with the oxide at maximum.

* Journal de Physique. Tom. 55, page 331. 1802.

"I will relate a few facts very suitable for rendering this view plausible, if they do not confirm it. In analyzing, as I have had occasion to do, some of the oxides of all degrees which Rinman has announced in calcining steel, iron, and cast iron, I have met with only two known oxides of this metal, mixed in different proportions; the ores yielded me only an equal mixture of black and red oxide.

"If we examine the greenish oxides which lead and bismuth give at the commencement of calcination, and those of tin, copper, &c., we only find an oxide at the maximum which envelopes different portions of the metal. If now we measure these degrees of oxidation by the nitrous gas which they give, we shall be led to believe that the metals oxidize themselves in all doses. But, no. There are unions of oxygen like those of sulphur, acids, &c. Election (elective affinity) and proportion are two poles, round which invariably move all the systems of true combination, whether in nature, or in the hands of the chemist. In a word, oxygen is not one of those bodies which can be mixed; when it combines, it subjects itself to certain proportions, and these proportions are what we have now to study."

1806. Vol. lxiii., page 367.

Defending Klaproth, he says, "A combination, according to our principles, Klaproth would tell you, is a sulphuret of silver, of antimony, of mercury, of copper; it is also a metallic oxide; it is a combustible body acidified; it is a privileged production to which nature assigns fixed proportions; it is an existence which nature has never created even in the hands of man, except with the balance in her hand *pondere et mensura*. Know then, he would add, that the characters of true combination are invariable as the proportion of their elements. From one pole to the other they are found the same under these two aspects, their physiognomy only may vary according to their mode of aggregation, but never their properties. No differences have been observed

between the oxides of iron of the south and those of the north. The cinnabar of Japan has the same proportions as that of Almaden. Silver is neither oxidated nor muriated differently in the muriate of Peru from the muriate of Siberia. In no part of the known world will you see two muriates of soda, two muriates of ammonia, two saltpetres, two sulphates of lime or of potash, soda, magnesia, or baryta, which are different; in fine, it is with one measure that all the combinations of the globe have been formed."

Page 370. "Nature has imposed certain laws of proportion in relation to those unions which we have come to call combinations."

Vol. lxiii., page 439.

"None of the researches which have been undertaken hitherto to assist the hypothesis of variable oxidations, even among the class of nonmetallic combustibles, have been able to discover above one or two of each; and each of these once oxidized is equally a product, the characters of which are invariable, preserving its properties with firmness on all occasions where they can be shown, whether free or in a state of combination. To this height are we now arrived in this branch of natural science."

Page 466. "There is nothing whatever in opposition to our extension of the same principles by regarding the solutions of sulphur, phosphorus, carbon, arsenic, zinc, &c., in hydrogen, not as simple solutions, without measure, in unfixed proportions, but as proportional combinations, as hydrurets of sulphur, phosphorus, &c., which the excess of the solvent may take into solution."

"Sur les Sulfures Métalliques."

Jour. de Ph., vol. lix., page 261, year 1804.

The following is a portion of the controversy between Berthollet and Proust. It begins with quotations from Berthollet.

"He (Proust) believes that there is attached to antimony

a dose of sulphur invariably fixed by nature, and that it is not in the power of man to increase or diminish it. He fixes this at 25 per cent."

Proust continues, "It is not I, but nature, or whatever power you choose, which places a barrier between it and all the efforts of every chemist who will attempt to make sulphuret of antimony above or below this proportion. *I have assigned no law to my discovery;* I have verified it only; I have followed the precept which Berthollet himself has traced in his profound work; when, says he, one substance combines with another, it is necessary to determine the proportions, and to examine the properties, &c. Such, in fact, has been the constant object of chemists, from the moment that they recognised that this determination was one of the most important bases of the history of combinations and of the science of analysis. Nobody can believe that nature will abandon her compounds to the chance of those variable proportions which Berthollet has chosen as the foundation of his system. But it is not the less true, that in proportion as the horizon of sulphurets extends, we do not see that the new facts which every day accumulates are of a nature to strengthen it."

Berthollet against Proust. "He has combined the oxide of antimony with different proportions of sulphur, and has obtained mixtures which may be represented by this formula; oxide +1+2+3, &c., of the sulphuret of antimony: has he not obtained there veritable combinations?" Page 262.

Proust: "To this, I shall reply, that solutions which have commenced, or which have not attained the term of saturation of which we consider them capable, ought to be viewed differently from combinations which are completed; but to explain myself, I illustrated those solutions in the same way I would do those of sugar in water, that is, as water +1+2+3 of sugar. I do not see that we can form more distinct ideas of the solutions of the sulphuret of antimony in its oxide. All chemists have hitherto believed that these glasses, livers, and crocuses,

were oxides which had been sulphuretted. The object of my work has been to show the fallacy of this, and that we must give up these sulphuretted oxides, which we admit, without proof, and receive in their place a species of combination, new, without doubt, but well demonstrated. Certainly this combination is in opposition to the ideas of Berthollet; he wishes to place them in the family of sulphuretted oxides, but it is no less certain, that those which I have announced do exist, and that they have this advantage over sulphuretted oxides, the existence of which is now terminated, that they afford the most natural solution of those thousand and one problems in antimony, the ridiculous nomenclature of which has shown the confusion of our ideas, and covered with obscurity the history of that metal."

Page 264. "To a ℔. of potash you add an ounce of arsenic; it is not saturated, you add two, you add three, it is not yet saturated, and so with more; but in waiting to discover the point of saturation, I repeat to them; your arsenical potashes are nothing at present but potash, plus one, two, or three of arsenic, but we have not time to prove that this combination will obey, as it no doubt will, the law of proportion, and shall not press you to decide on it. These are results so variable that they destroy your laws of proportions, and render your apothegms illusory. Berthollet is too just not to agree that the series of numbers by which I have sought to represent the solutions of the sulphuret of antimony in its oxide, has not the least relation to that which I have hitherto called proportion in combinations."

Journal de Physique, 1805, vol. lix., page 321.

"Sur les Oxidations Métalliques."

"I ought to explain, says Berthollet, that the proportions of oxygen may vary progressively after the limit or the combination becomes possible, until it attains the last degree; and when this does not take place, it is because the conditions

which I have indicated become an obstacle to this progressive action."

"A little before this are to be found the facts on which this illustrious chemist establishes the theory of progressive oxidations, which he opposes to that which I have given out on different occasions, and of which the base is, that the combustible bodies are arrested at fixed terms of oxidation, in the same way as we see to be the case with sulphur, phosphorus, carbon, azote, and the greater part of the metals."

Page 328. After shewing that the fine lead powder got by shaking the metal in a bottle is a mixture of metal and oxide of lead, and not a low and varying degree of oxidation, he adds;

"But is each of these molecules, one might say, suddenly at one leap elevated from 0 to 9, to 12, to 25 per cent? And is it possible that they do not pass successively all the ascending terms which the imagination can conceive possible between the two extremes! I reply, that it is impossible in the actual state of things to say if the oxidation follows or does not follow this progression, because in the calcination of a metal, of lead for example, the senses are not struck with any phenomenon which can guide the judgment in the choice between two opinions; but although we do not see intuitively that which occurs actually in calcination, we are not hindered from judging clearly by the aid of numerous analogies which the field of combination offers."

Page 330. "Let us mix the green sulphate of iron with the red, each base will hold its own amount of oxygen, and there will be no conciliation between the two, no division which will bring forward to us those intermediate oxidations which the mind would desire to discover. Is it a piece of iron which we throw into the red sulphate? We see the base of the salt descend to 28, not by a retrograde march which arrests each of these particles at 47, at 46, at 45, &c. of oxidation, but by the instantaneous lowering of each from the limit (or term),

48 to 28, and this is well confirmed by analysis, because we discover in the solution nothing but red molecules mixed with green ones."

Page 334. "I will say then of the sulphurets as I said of the oxides, there are only two of them."

Speaking of the oxide of copper:

Page 351. "If we establish the calculation on this basis, we find that a quintal of yellow oxide is composed of 86 of copper and 14 of oxygen, whilst the black oxide to which we wish to compare it, contains only 80 of metal and 20 of oxygen; or in other words, if copper condenses 25 per cent. of oxygen to raise it to its maximum, it condenses only 16.3 to raise it to its minimum. Here then, we find, as in all other combinations of oxygen, new reasons for recognising this law of nature, which subjects the metals and combustibles to those proportions from which we cannot separate them, however various may be the circumstances under which they have united."

It really is a melancholy thing to read these papers of Proust. He had advanced by the most careful steps to the conclusion, that all combinations were made in proportions defined by some law of nature, that they were weighed and measured before they were united, and yet failed to see a law. Richter used the words from the Septuagint, God has made all things by measure, number, and weight; and Proust uses a similar phrase with his "pondere et mensura," from the Vulgate, leaving out the word number, which he did not sufficiently see. He saw, with great clearness, that without such constant proportion the products of nature would lose their stability, and the characters of bodies could not be depended on for permanence. We have here no difficulty in judging how much he did, and how much he left undone; how far his own mind was advanced, and how it had merely speculated. He tells us all distinctly. When he uses +1+2+3 of proportions, he tells us it is merely for illustration, he did not

mean it to indicate the order of combination, he had in fact made no theory, at least found no law on the subject, although he clearly saw that it must be owing to some law of nature. He fought for constant proportions in combinations, and fought well, but he had no idea of a constant quantity of oxygen found uniting with a constant quantity of every metal, and making higher oxides by steps always of an equal altitude, although he proved that the rise was not that of an inclined plane, but by "fixed terms." And yet it follows as a consequence, so closely in fact does it follow, that we must put ourselves in the position of the early chemists of the century well to understand the difference. When we have taken that position, we then see how thin was the veil, although utterly impenetrable, that divided his opinion from the present, and prevented the acute, active, and logical mind of Proust from attaining to the great discovery. His determinate proportions are given as remarkable facts, in connection with which he confessed to perceive no law.

Had such men studied Higgins, we should probably never have heard of this controversy, but he was not studied. We may therefore learn not so readily to blame a man for want of honesty, when he publishes for a discovery what has been known before. The most indefatigable workers of the period had neither read Higgins nor Richter. Besides, scientific men like other men are led by fashion, the follies of some men become great discoveries for a while, and the wisdom of men comparatively obscure, such as the two mentioned, is neglected or sneered at. Yet the whole body endeavours to acknowledge facts only.

As a specimen of the chemical books of the time, let us take one published in the same year as Dalton's work on the atomic theory, although after Dr. Thomson had made it public, "A Course of Theoretical Chemistry, by Friedrich Stromeyer." *

* Grundriss Theoretischer Chemie zum behuf Seiner Vorlesungen entworfen von D. Friedrich Stromeyer. Göttingen, 1808.

He says, p. 66, § 36, "The affinity of a substance towards another is always in proportion to its chemical mass, which it brings with it for combination."

Page 68. "Affinity is consequently in no way an elective attraction, in the sense in which Bergman and his followers have taught it."

Page 80, § 59. "By means of affinity alone two substances may combine with each other in every quantitative proportion. Although this asserts quite the opposite of Bergman's theory, and even appears to contradict experience, it is nevertheless a perfectly natural consequence of the above.

"For suppose A and B could combine in the proportions of 5 : 7, and not as 10 : 7, if we put 7 parts of B with 10 of A, then the very same thing would happen as when we put 7 parts of B with 5 of A; that is, the 7 parts of B would combine with 5 parts of A, and the other 5 parts would remain uncombined.

"If this were really possible, then it would follow clearly that 10 parts of A contained no more affinity than 5 parts, which however completely contradicts what is proved in § 36."

The idea of the present law seems to have entered into his mind as a conclusion to be avoided for its absurdity.

§ 60. "But as soon as the power of cohesion or expansion of two substances of a given mass, acting on each other, ceases to be overpowered by their affinity, an exception constantly takes place to this universal law, and a certain proportion of mixture is established between the two substances, which, on account of their affinity, can no longer be surpassed, and consequently limits their mutual affinity.

"But if there were a removal of the obstructions caused by cohesion and expansion to the affinity of two bodies, then the law would again come into action in its first unity."

"The absorption of several gases by water, the solution of salt in water, and the oxidation of metals, best establish that which has been said."

§ 61. "This general law is subject to an exception also, when there is a change of the aggregate condition during the action of two or more substances, and in this case also a combination takes place of a fixed proportion."

Here we have a clear account of the direction that Berthollet's teachings gave, and the consequences are logically deduced. Here, too, we find that fixed proportions are obtained as exceptions, but it is also seen how needful it was to have them occasionally in order to explain facts in the science.

Stromeyer's words do, in fact, represent the confused and contradictory opinions of the time, and afford us another proof that no one before Dalton had given opinions sufficiently authoritative on the atomic theory to be retained by the teachers of chemistry; and we may add also, none deemed of sufficient importance to demand at the time very serious discussion.

This want of attention, even to imperfect theories, arose mainly, I believe, from the fact, that those theories hitherto given had not had accumulative scientific proof to give them force in the world; they had organized no executive force. Chemists, generally, had not arrived as far as the inquiries already quoted.

It was intended to give a number of similar instances, to show how entirely all atomic theories and theories of definite proportions were out of the boundaries of general chemical science when Dalton published. I happened to take up Stromeyer first; many instances occur in our own country, but this will probably be found sufficient.

CHAPTER XI.

DALTON'S ATOMIC THEORY.

We now come to Dalton, whom we have left since he first thought of weighing atoms; this has been done in order that by accumulating all the materials he might have used, we may know what has been his especial work. Matters stood thus: Higgins had, in 1789, seen the fundamental principle clearly, and given it out more as a thinker than a discoverer, neglecting to generalize; that principle included simple proportion or the law of necessary definite composition and multiple proportion. Richter had systematized all the laws of combination known to him, but had not known of Higgins, although he would there have got the clue to all his strivings. He discovered one of the most important consequences of the fundamental law, in reciprocal proportion, but did not rise up to first principles. Scientific men could get no decided guide from either, and preferred to follow Berthollet, who was leading them out of the right direction, obstructing for many years the advance of chemical philosophy, or compelling others to accumulate proof until it was sufficient to overwhelm him.

The materials we have for tracing the progress of Dalton's opinions are few, but distinct and sufficient. I shall not enter into the argument of honesty, which can be thrown with greater violence at so many heads, but shall take it for granted, that all those who have had the honour of working at the laws of chemical combination with any success, have also had the advantage of an honest mind. I know of no dishonesty on any side among the principals connected with this subject, and their defenders have erred, probably, on account of proving one side better in morals than the other.

From the earliest period of his scientific life Dalton had been accustomed to think carefully on the constitution of the atmosphere; this is seen as early as 1793, in his meteorology. This subject continued to be a favourite one, and led him to gases generally. The experiments quoted at p. 43, on nitrous gas and oxygen, and those mentioned afterwards in a quotation from Dr. Thomson, shew the method by which he came to believe, and to prove experimentally, the existence of definite and constant proportion.

Here lies the difference between him and Higgins. Higgins expressed the fundamental idea as clearly as Dalton, but it was still left uncertain. Dalton proves it by experiment, draws the conclusion, and tells us the "theory" in a few lines. We have then distinctly the method by which he came to believe in definite proportion and multiple proportion. He proved them for himself, and theorized for himself. No books of any writers before him, no Proust or Berthollet controversies were so decisive as these few experiments of Dalton; not clearer than the words of Higgins, but more decisive, because the result of observation and of reasoning combined. This seems to have been his first direct entrance into the region of the atomic theory.

In reading over his earlier works, or even in reading the short account here given, we may remark with what a firm grasp he lays hold of the existence of atoms, of the idea that all matter is made up of separate ultimate particles, divisible or indivisible. We find no scientific man holding the idea with such firmness; to others it was a theory, to Dalton it was a fact, which he could not conceive otherwise. We find that even the air is represented* as an agglomeration of bodies heaped up like piles of shot. We appear to be entirely removed from the region of speculation when reading his words; although he leads us farther than the most fantastic speculator had done, the road is made so clear before us, that

* Page 47.

we find no difficulty, either physical or metaphysical. He persuades us to go, leads us and describes all to us in a few sentences, when volumes of persuasion written before had not been sufficient to induce men to turn their eyes. His descriptions are rigid as well as picturesque. Some persons would apply to them the word material, and still more the word mechanical. It was by following rigidly the *mechanical* properties of his atoms that he arrived at his results. To those who read his works, it will be clear that his mind became gradually more confirmed in this course.

In reading what he says, at p. 49, we see him plainly verging towards his theory, and also the nature of his struggle, which is in no respect similar to that of any other inquirer. He there first says, that he is inquiring into the relative weights of the ultimate particles of bodies. This idea had never suggested itself as practical to any one before Dalton, nor am I aware that it has ever been claimed. Having made some advance in this inquiry, he made it the starting point of all that he advanced on atomic chemistry and the theory of proportion.

He was not in haste to publish his theory, but told it openly to Dr. Thomson, in 1804; this, then, is the date of the complete discovery, as Dr. Thomson published an abstract of it at once. Some persons unacquainted with this have advanced an argument against him, his own book containing the subject not having been published till 1808. A well known quotation from Dr. Thomson's history says, "Mr. Dalton informed me that the atomic theory first occurred to him during his investigations of olefiant gas and carburetted hydrogen gas, at that time imperfectly understood, and the constitution of which was first fully developed by Mr. Dalton himself. It was obvious from the experiments which he made upon them, that the constituents of both were carbon and hydrogen, and nothing else. He found, further, that if we reckon the carbon in each the same, then carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas does.

This determined him to state the ratios of these constituents in numbers, and to consider the olefiant gas a compound of one atom of carbon and one atom of hydrogen; and carburetted hydrogen of one atom of carbon and two atoms of hydrogen. The idea thus conceived was applied to carbonic oxide, water, ammonia, &c., and numbers were given representing the atomic weights of oxygen, azote, &c., deduced from the best analytical experiments which chemistry then possessed."

His first atomic weights, already given,* were published in 1803; he did not publish his "New System" till 1808. He says then, † "A pure elastic fluid is one, the constituent particles of which are all alike, or in no way distinguishable. * * * These fluids are constituted of particles possessing very diffuse atmospheres of heat, the capacity or bulk of the atmosphere being often one or two thousand times that of the particle in a liquid or solid form. Whatever, therefore, may be the shape or figure of the solid atom abstractedly, when surrounded by such an atom it must be globular; but as all the globules in any small given volume are subject to the same pressure, they must be equal in bulk, and will, therefore, be arranged in horizontal strata like a pile of shot."

The chapter "On Chemical Synthesis" gives his theory. He there says, "When any body exists in the elastic state, its ultimate particles are separated from each other to a much greater distance than in any other state; each particle occupies the centre of a comparatively large sphere, and supports its dignity by keeping all the rest, which, by their gravity, or otherwise, are disposed to encroach on it, at a respectful distance. When we attempt to conceive the number of particles in an atmosphere, it is somewhat like attempting to conceive the number of stars in the universe; we are confounded with the thought. But if we limit the subject, by taking a given volume of any gas, we seem persuaded that, let the divisions be ever so minute, the number of particles must be finite; just

* Page 49. † "A New System of Chemical Philosophy." Part I., p. 145.

as in a given space of the universe, the number of stars and planets cannot be infinite."

"Chemical analysis and synthesis go no farther than to the separation of particles one from another, and to their reunion. No new creation or destruction of matter is within the reach of chemical agency. We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen. All the changes we can produce consist in separating particles that are in a state of cohesion or combination, and joining those that were previously at a distance.

"In all chemical investigations it has justly been considered an important object to ascertain the relative *weights* of the simples which constitute a compound. But unfortunately the inquiry has terminated here; whereas from the relative weights in the mass, the relative weights of the ultimate particles or atoms of the bodies might have been inferred, from which their number and weight in various other compounds would appear, in order to assist and to guide future investigations and to correct their results. Now it is one great object of this work to shew the importance and advantage of ascertaining *the relative weights of the ultimate particles, both of simple and compound bodies, the number of simple elementary particles which constitute one compound particle, and the number of less compound particles which enter into the formation of one more compound particle.*

"If there are two bodies A and B which are disposed to combine, the following is the order in which the combinations may take place, beginning with the most simple: namely,

1 atom of A+1 atom of B=1 atom of C, binary.
1 atom of A+2 atoms of B=1 atom of D, ternary.
2 atoms of A+1 atom of B=1 atom of E, ternary.
1 atom of A+3 atoms of B=1 atom of F, quaternary.
3 atoms of A+1 atom of B=1 atom of G, quaternary.
&c., &c.

"The following general rules may be adopted as guides in all our investigations respecting chemical synthesis.

"1st. When only one combination of two bodies can be obtained, it must be presumed to be a binary one, unless some cause appear to the contrary.

"2nd. When two combinations are observed, they must be presumed to be a binary and ternary.

"3rd. When three combinations are obtained, we may expect one to be a binary, and the other two ternary.

"4th. When four combinations are observed, we should expect one binary, two ternary, and one quaternary, &c.

"5th. A binary compound should always be specifically heavier than the mere mixture of its two ingredients.

"6th. A ternary compound should be specifically heavier than the mixture of a binary and a simple, which would, if combined, constitute it, &c.

"7th. The above rules and observations equally apply when two bodies such as C and D, D and E, &c., are combined."

* * * * * * *

"In the sequel the facts and experiments from which these conclusions are derived will be detailed, as well as a great variety of others, from which are inferred the constitution and weight of the ultimate particles of the principal acids, the alkalis, the earths, the metals, the metallic oxides and sulphurets, the long train of neutral salts, and in short, all the chemical compounds which have hitherto obtained a tolerably good analysis. Several of the conclusions will be supported by original experiments.

"From the novelty as well as importance of the ideas suggested in this chapter, it is deemed expedient to give plates exhibiting the mode of combination in some of the more simple cases. A specimen of these accompanies this first part. The elements or atoms of such bodies as are conceived at present to be simple, are denoted by a small

circle, with some distinctive mark; and the combinations consist in the juxtaposition of two or more of these; when three or more particles of elastic fluids are combined together in one, it is to be supposed that the particles of the same kind repel each other, and therefore take their stations accordingly."

In the figures to which he refers above he has shewn to us how vividly he formed these ideas, that they were no mere fancies which had passed through his brain, but distinct impressions, ready prepared for utterance. No doubt is left upon our minds as to his opinions, which are, that every piece of matter, even the smallest, must follow the laws of the largest; that when pounds of matter unite, the atoms contained in them must unite also, until we come to the fact that only atoms can really be said to unite. Now as the conception of any fraction of an atom is a contradiction and impossible, they must constantly unite as wholes, and the proportion will be constant. If constant in the smallest quantities, then so in the largest, explaining the permanency of the constitution of bodies so much disputed, and making it a law of nature. If two compound bodies unite, the same law is followed out.

He then gives instances of combination, and adds to his explanation a plate of the "arbitrary marks or signs chosen to represent the several chemical elements or ultimate particles."

He gives twenty atomic weights and seventeen analyses of gases and acids. His atomic weights are—

Hydrogen, its rel. weight..	1	Strontites	46
Azote	5	Barytes	68
Carbone	5	Iron	38
Oxygen	7	Zinc	56
Phosphorus	9	Copper	56
Sulphur	13	Lead	95
Magnesia	20	Silver	100
Lime	23	Platina	100
Soda	28	Gold	140
Potash	42	Mercury	167

"An atom of water or steam composed of one oxygen and one hydrogen, &c. = 8," and so on with other bodies.

This is the result of enormous labour, added to that of the many gaseous analyses. Let no one say that because the atomic weights are in most cases inexact he shews want of power. We have seen in our own days how difficult it is to get an exact atomic weight, we have found that it needs the combined forces of several laboratories to settle one to satisfaction, and we must rather admire that man who approached first so near. But, although Dalton has been called a rough worker, and I am not prepared to deny it, we must remember that his analyses are not behind the time, but in advance of it in early life. At the period when he was working out this theory, the analyses of all chemists were in general only approximative. Fine analysis was only then beginning its course. But as Dalton says, "it is not necessary to insist on the accuracy of all these compounds, both in number and weight; the principle will be entered into more particularly hereafter, as far as respects the individual results."*

He also says, "it is not to be understood that all those articles marked as simple substances, are necessarily such by the theory;" *ex. gr.*, soda and potash are mentioned as compound.

In various parts of his work we learn exactly the method in which he applied his theory, and as he devoted his time to its illustration, we are not left in any doubts as to his opinions.

We have now to find exactly what new ideas he produced. We have seen in the last chapter the state of chemical opinion, the prevalence of the Berthollet philosophy, and the uncertainty hanging over the opinions opposed to his. General opinion on combination was in reality not more advanced than in the earliest days of the science. Dalton found matters in this state of confusion, and we have seen the

* Page 220.

results he arrived at, and the process. 1st. By long reflection on the constitution of bodies, especially of gases, he became convinced of the necessity for ultimate particles, divisible or not so. These particles unite together, and form of course a definite compound. If the smallest part is definite, so is the largest. This is the fundamental law of definite compounds. 2nd. Various numbers of atoms may unite—there may be one, two, three, or more—there can be no division of atoms. As large bulks are constituted just as small are, so multiple proportion becomes a law by which bodies are constituted. 3rd. Compound bodies constituted as the above unite particle to particle in a manner exactly similar to simple bodies, and so we have compound proportion, and are led to a mode of inquiry into, and a method of expressing the most complicated bodies. 4th. If we obtain the relative weights of the constituents of bodies we obtain the relative weights of the atoms, because the smallest parts must be constituted as the largest. 5th. The relative weights of the atoms become constant expressions for the proportions of combinations.

These are the fundamental principles which made chemistry a science and hold it together, and although Dalton had no direct help in discovering any of them, we have seen that Higgins had already expressed the first two. Richter and Fischer had made out numbers representing the reciprocal proportion of bodies, and although not going to first principles, and establishing no law, Fischer's numbers adopted by Richter, I believe in 1803, are really atomic weights or equivalents, although they did not see them to be such.

There existed, therefore, in the world material for completing the theory of combination, but there was no one who saw it clearly, and no one who knew both parts published. Dalton cannot be blamed for not knowing them; no one knew them. Although Dalton had to begin without their aid, the custom of the world is to give credit to him who adds to its accumulated knowledge, not to him who obtains knowledge in

his closet merely. By this custom, therefore, Dalton's credit would be the whole theory, minus what Higgins and Richter had done; how much this was who will venture to declare, the two parts being the conceptions of different brains, so that Thomson, with his great quickness, could make nothing out of one, and Berzelius, with his patience, nothing out of the other; nevertheless, Dalton must not have their credit, and it was not the habit of his independent spirit to seek from others. Dr. Henry has made it clear that he had not seen Higgins's book. He no doubt saw Proust's results. They are in the library of the Manchester Society, and they are probably the most likely to have assisted him, as the analyses and reasoning are remarkably clear, but devoid, as before said, of theory; still the clearest and best were later than Dalton. This, therefore, seems to be the result, that although actually producing all the theory within himself, from the world his deserts are that he first saw the great importance of the idea of using atoms to illustrate proportion and definite constitution. He followed up the idea, and found in it a fundamental natural law, as it appears hitherto. He saw the use and importance of multiple proportion, or the adding of atom by atom in twos or in threes, and he proceeded to investigate nature under this impression. He proved that bodies followed laws, such as fitted his hypothesis, which was thenceforth taken into the province of scientific theory. To perform the above it was needful to grasp the idea more firmly than it had been done, to work laboriously, and to decide convincingly. This Dalton did. He then extended this from simple to complex combinations, and gave the first idea as well as proof of compound proportion, laid down the laws orderly into a system, and accompanied the whole by abundant and laborious proofs. He gave the first idea of atomic weights. Under this head came Richter and Fischer's numbers. Richter grappling with those numbers never could obtain a rational theory from the phenomena. Dalton's plan explains these

numbers with the greatest ease, and looks on such as a necessity of the fundamental law, instead of the beginning of the inquiry as it was to them.

It seems to me, then, that what happened historically, happened also intellectually. Dalton had included his predecessors in his more extensive system. He had gone to the summit of the hill, and when coming down, found proofs that they had been making good progress upwards. Higgins had gone at once to the top, as it appears to me, but took no heed to make the needful observations when he was up, or he found the prospect entirely obscured. We are compelled to put reciprocal proportion in a secondary position, as it seems to me it cannot be called a law, but one of the consequences of a law; and the evidence brought to support it, otherwise than empirically, presupposes some of the principles on which the general laws depend.

It was by a careful mechanical juxta-position of parts that Dalton arrived at his idea, it is eminently mechanical, and it is remarkable that all progressive views on that subject have been so. *He introduced proportional weight into the theory, and found it to agree with facts.* His is, therefore, the *quantitative* atomic theory. In this complete form no one seeks to take from him the honour. The total is so entirely his, that the disputed parts can be held only as a fealty.*

Although Dalton rigidly held to the idea of atoms, he by no means supposed that we had attained the indivisible atom

* Dr. Schweigger, in his pamphlet, objects that Dalton's theory was not conceived in the spirit of the ancient theory, because he allowed some atoms to be small and some large. Strange this. The reason that the ancient theory is insufficient, is simply because it was not conceived in the spirit of Dalton's. 2nd. Most of the ancients allowed greater and smaller atoms, and various shapes. 3rd. Dalton formed his theory in the belief that atoms were of one size, but afterwards saw reason to change his opinion. Dr. Schweigger, therefore, has forgotten the opinion of Dalton and the ancients, as well as Richter's preface, where he calls the permanent neutrality of neutral salts after neutral decomposition, a well known fact. He sneers at the atomic theory, thinking that by putting it down, Dalton will fall; I don't agree with him there, but there is time enough to wait.

in our elements; at least, he expressly reserved this point. What he speaks of is simply the ultimate particle that seems to act in our chemical processes.

Dalton used atom and particle. Many have objected to both, but they are words which really involve less theory, and are more generally applicable than any yet obtained, except, perhaps, combining proportion, which is too long. Equivalent, Wollaston's word, is itself too long, limited in its meaning at best, and at times either meaningless or incorrect. Whenever we conceive of combining proportion, we reduce the quantity to the smallest conceivable, every portion becomes a unit, and each unit is undivided; it is a particle for the moment, and an atom as far as the effect we study is concerned. Having, after much labour, attained this mode of thinking, we can now scarcely think of compounds otherwise, nor indeed has any other method suggested itself as better, although investigation is promising advance in our knowledge, and will probably some day astound us by its results.

The consequences of Dalton's laws gradually shewed themselves to be, that there was now one great law or theory in chemistry, so that it was for the first time fit to be called a science. Heretofore it was a series of separate facts, and even now we may say that it is more a branch of natural history than of exact science, but as a science it is preserved by this fundamental law and its branches, and by this only. It is stretching itself out in many directions, and its future will undoubtedly be still more brilliant; at present, although it has planted its standard in numerous spots, it has fixed a government on few, but still issues the central law unchanged, although with explanatory extensions. The history of the atomic theory, since Dalton's time, is contained in Dr. Daubeny's work, and on that it is not my desire to encroach, nor could I hope to equal it.

A very little need be said as to the consequences of the promulgation of Dalton's views. They were explained in

1804 to Dr. Thomas Thomson, the very learned professor of chemistry in the University of Glasgow, and by him they were inserted in his system of chemistry. But it was not a truth that could electrify the world, it was on a subject on which few thought, one of which many said, "what is the use of it?" that miserable question which occurs to men to whom the revelation of God's truth is of no interest, unless an immediate advantage is promised. Dr. Thomson (Nicholson's Journal, Vol. 21, p. 87) says even four years after, "This curious theory, which promises to throw an unexpected light on the obscurest parts of chemistry, belongs to Mr. Dalton;" shewing that even then it required to be taught even to chemists. We find in reality that for years afterwards it was to most chemists a mere speculation.

In a paper by Wollaston in the same volume, p. 164, he says, "Dr. Thomson has remarked that oxalic acid unites to strontian as well as to potash in two different proportions, and that the quantity of acid combined with each of these bases in their superoxalates is just double of that which is saturated by the same quantity of base in their neutral compounds. As I had observed the same law to prevail in various other instances of superacid and subacid salts, I thought it not unlikely that this law might obtain generally in such compounds, and it was my design to have pursued the subject with the hope of discovering the cause, to which so regular a relation might be ascribed.

"But since the publication of Mr. Dalton's theory of chemical combination, as explained and illustrated by Dr. Thomson, the inquiry which I had designed appears to be superfluous, as all the facts that I had observed are but particular instances of the more general observations of Mr. Dalton, that in all cases the simple elements of bodies are disposed to unite atom to atom singly, or if either is in excess, it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms."

It is plain that Wollaston, who was behind in no theories, was unable to obtain from the knowledge of the times an explanation of these phenomena, even when he saw the difficulty clearly; and it is plain also, that although it was said that he would have discovered it had Dalton failed, that he did not discover it even when he had in his hands as many facts as Dalton had, and a greater power of accurate workmanship. The genius was wanting, the acuteness of Wollaston and of Proust could not penetrate, where the simplicity of Dalton was at home.

In Sir Humphrey Davy's Bakerian lecture for 1809, in giving some analyses, he says, "the same proportions would follow from an application of Mr. Dalton's ingenious supposition," but even he, with a mind much more capable, as we might suppose, of delighting in grand general laws, such as from their brilliancy come upon us like the finest poetry, even then saw little in the new theory, and said little upon it. When it began to be established he was inclined to prove that Dalton was not the first discoverer, although when he delivered his discourse on giving Dalton the gold medal of the Royal Society he had arrived at a clearer view of the subject. The same slow vision was, as might be more readily conceived, the case with Berzelius, who took it gradually up as it came from his own analyses, growing during his whole life into more and more absolute certainty. But Dalton had given a table of atomic weights in 1803, and for years these philosophers plodded after him with numberless proofs, Berzelius surpassing all others in accumulating and arranging scientific wealth.

It will not be pleasant to review the delinquencies of our great men, nor the slowness of their conceptions, their desire to limit the law, and to cramp it down to the bounds of their own knowledge, denying its power to explain more than they had seen. Scepticism has its work to do in the world, and credulity has had many victims, but it may well be said that the amount of knowledge to be gained is so great that our

capacities for belief must enlarge themselves rather than diminish, and we shall be left behind with a small and inadequate supply of intellectual food, if we refuse to take it until we have extracted its one-tenth per cent. of questionable adulteration.

The law has established itself; it is true. Our knowledge of its ramifications will increase. If it had not great ramifications, we might question its own intrinsic greatness. Isomorphism and isomerism are two of these, beautiful in every respect, thoroughly beautiful. But they are no contradictions, the number of provinces do not prove the smallness but the greatness of a kingdom. Why then should they even be mentioned as modifications, they may be said even to be necessary results. Allotropy is itself a curious and beautiful fact, and one that we may readily suppose will widen itself out, perhaps even till alchemy itself shall cease to be wonderful; but it does not disprove the atomic combinations.

We may call the ultimate particles which practically unite *equivalents*, as Wollaston did, but we don't alter the fact, we only chronicle our hesitation, and substitute a name which cannot be final, as it represents only a temporary theory; it only says that the quantities are equivalent to each other, but refuses to decide what these quantities are. We may call them volumes, like Berzelius, but we find then that we go on a wrong hypothesis, as the atomic volumes are not the same as their weights in every condition, whatever might be the case if all were gaseous.

We have, in fact, found no name representing the case as well as atom, and, giving due limits to the meaning of the word, it represents the state of belief in the mind of every chemist, whilst no fact whatever bears directly against it.

At the same time I do not mean to advocate the atom with the physical constitution given to it by Dalton, as well as by Newton and the ancients, not being able to see it possible;

but this is not a place for my own views: I have referred to what may be called the practical atom, or the smallest amount that unites.

Is then chemistry scientifically disposed of by this theory? as well might we say that Newton exhausted the heavens of its knowledge. Year after year will furnish us with marvellous truths, nor can we believe that centuries or millenaries will exhaust God's wisdom in the earth. Already has Davy's aluminum, a brittle useless powder not quite pure, turned a beautiful metal, and the slippery mud of a clayey soil been assimilated to "shining silver." The atomic theory may be further analyzed, and under its simple laws may be found another which will not only include all we now have, but a host of others still unsuspected; the time may even come when a new chemistry will be revealed to us, a world under our present elements, when every element will be convulsed and shaken into fragments, by powers which nature will put into our hands; but even that does not destroy the laws of the present. Even when that scientific convulsion comes, we can scarcely doubt that the elements will break up, well proportioned and according to regular laws, if they break into fragments at all. But this stratum of our knowledge cannot be annihilated by any under stratum; what we have found is true, whatever higher truths may overpower us with their splendour. When these truths come let us receive them openly and willingly, giving them encouragement instead of envious repulsion, knowing, in fact, that they must come, and rather let us make an occasional mistake in harbouring a mere mortal, than lose the opportunity of an angel for a guest. There are incredulous fools who have made the world's throbbing heart a blank to them, lest they should perhance at times be cheated. There have been madmen who have refused to eat, lest they should be poisoned.

These reflections naturally arise on considering the manner in which Dalton's discovery was reviewed. It cannot be denied, however, that the time being nearly ripe was the

cause of its ultimate success. To discover out of place and out of time is a great misfortune. To be before the age in knowledge is to a man a curse, and to a generation no advantage; at least it would seem so, although there may be a value even for this occasional misplacement not to be lost sight of in estimating man's progress towards a higher civilization. But in such cases the individual is not appreciated, the generation does not hear him, his years pass by in misery, and his attempts to teach are a failure. That knowledge is best rewarded which is a fit evolution of the age, and which can be at once put to use for practical purposes, or for mental cultivation. It behoves us then to be respectful towards new opinions, and tender towards crotchets, lest we may be laughing, as so many have done before us, at beautiful truths. The rudiments of truth are no more beautiful to us than the roots of flowers, until we study them thoroughly; by forgetting this men are made victims who ought to be revered.

CHAPTER XII.

LATER LIFE OF DALTON.

ALTHOUGH the atomic theory must ever be considered as Dalton's great discovery, we find that he obtained it at the end of a series of investigations of themselves sufficient to have made him a conspicuous character. These earlier labours had great influence in advancing physical knowledge, and first brought him into repute. We find him so early as 1804 lecturing in the Royal Institution, when his theory was scarcely known but to himself, and when it was not expected to form part of a lecture. Fame was now beginning to hover round him, and he was not insensible of the change. But to persons of his habits, fame is less welcome in person than by letter, as we may say. They are less fitted for receiving the compliments of the world addressed to them in their presence, than for receiving those more impressive results,—great respect and deference; and, above all, the pleasure of seeing the influence they exercise upon society. Dalton was evidently conscious of the position to which he was entitled in the scientific world, although equally conscious that he was out of place in those brilliant assemblies in which scientific men in capitals occasionally mingle. He seems to write as if it were rather curious, although true, that he, too, was one of them, that the hope and struggle of his life to attain a position in science had been realized, but to feel at the same time that his life was not there, but as before, in his laboratory.

It not unfrequently happened, after this period, that he was engaged in controversy; and we find him there acting with great ease and deliberation, always without fear. Like most scientific men, he was destined to modify or to contradict

some of his earlier conclusions, having found that he formed laws too hastily. At page 9 of his "New System," he says, "Sometime ago it occurred to me as probable, that water and mercury, notwithstanding their apparent diversity, actually expand by the same law, and that the quantity of expansion is as the square of the temperature from their respective freezing points. Water very nearly accords with this law according to the present scale of temperature, and the little deviation observable is exactly of the sort that ought to exist, from the known error of the equal division of the mercurial scale. By prosecuting this inquiry, I found that the mercurial and water scales divided according to the principles just mentioned, would perfectly accord as far as they were comparable; and that the law will probably extend to all other pure liquids; but not to heterogeneous compounds, as liquid solutions of salts."

In Vol. II.* he says, "The great deviation of the scales between the temperatures of freezing water and freezing mercury is sufficient to show, as Dulong and Petit have observed, that their coincidence is only partial:" and so acknowledges his error.

In the first volume † he also said, that "the force of steam from pure liquids, as water, ether, &c., constitutes a geometrical progression to increments of temperature in arithmetical progression." Also, "that the expansion of permanent elastic fluids is in geometrical progression to equal increments of temperature." Again,‡ "that the force of steam in contact with water, increases accurately in geometrical progression to equal increments of temperature, provided these increments are measured by a thermometer of water or mercury." This seemed an ingenious group of laws, and although he gave them up, yielding to the results of Dulong, he published them unaltered in the edition or reprint of 1842. But that

* Page 289. † Page 13. ‡ Page 11.

was his custom, even when his opinion changed; it was more with a view of obtaining copies of his own works, than with the view of continuing any law proved to be incorrect, as appears from the preface to his Meteorology.

In his "New System," Part 1st, he treats of heat and the constitution of bodies; in Part 2nd, which was published in 1810, he treats of the chemical elements. In these volumes it is remarkable how thoroughly every idea has been revolved in his own mind, and become his own, before he has ventured to write it. Every chapter shows a strict independent thought, but, on the other hand, the book is wanting in the results of others, and could never consequently be a complete system. He endeavours to construct the whole science himself, more than could be accomplished by any man. The book is written in a more attractive manner than systems of chemistry now assume; and there is a constant discussion of questions which give an insight into the state of knowledge of the time and the tendency of chemistry. Still the arrangement was not well adapted for the young student, although the work was a great fund of thought for the advanced man of science. Even now few will be able to read it without advantage.

Part 2nd is principally taken up in determining the composition of bodies and their atomic weights. In the appendix he enters into discussion with Gay Lussac. He there says, Gay Lussac's "opinion is founded upon a hypothesis that all elastic fluids combine in equal measures, or in measures that have some simple relation one to another, as 1 to 2, 1 to 3, 2 to 3, &c.; in fact, his notion of measures is analogous to mine of atoms; and if it could be proved that all elastic fluids have the same number of atoms in the same volume, or numbers that are as 1, 2, 3, &c., the two hypotheses would be the same, except that mine is universal, and his applies only to elastic fluids. Gay Lussac could not see that a similar hypothesis had been entertained by me and abandoned as untenable." In this he refers to the following in p. 188 of

Part 1st. "At the time I formed the theory of mixed gases I had a confused idea, as many have, I suppose, at this time, that the particles of elastic fluids are all of the same size; that a given volume of oxygenous gas contains just as many particles as the same volume of hydrogenous." But he arrived at the conclusion, "That every species of pure elastic fluid has the particles globular, and all of a size; but that no two species agree in the size of their particles, the pressure and temperature being the same." Then he concludes, in Part 2nd, "The truth is, I believe, that gases do not unite in equal or exact measures in any one instance; when they appear to do so, it is owing to the inaccuracy of our experiments. In no case, perhaps, is there a nearer approach to mathematical exactness, than in that of 1 measure of oxygen to 2 of hydrogen; but here the most exact experiments I have ever made, gave 1.97 hydrogen to 1 oxygen."

This discussion brings out prominently some of the points of Dalton's character. He objected to the idea of bulk being taken as a combining proportion; it was his great object to show the importance of weight and the completeness with which it answered every purpose. He conceived the combination by bulk as accidental, evidently because he had not examined the relations of the subject with sufficient care, and probably with some aversion, as his own discovery seemed in question. He is strict in the examination of the analyses of others, and seeks mathematical precision, when he could so very easily, in his own researches, overleap a few per cent. Yet no one would have been, on theoretical grounds, so likely to arrive at Gay Lussac's law as Dalton himself, as he paid most attention to the bulk of atoms, giving the relative diameters of particles of the different gases after giving the relative specific and atomic gravity. He did not always obtain an analysis so correct as that mentioned above, he continually, and to the last, insisted on the atomic weight of oxygen being 7; and he gives the specific gravity as 14 times that

of hydrogen, and as he gave the specific gravity of oxygen at 1.127, he was compelled to raise very high that of hydrogen, which he made .0805. We should have supposed Dalton to be, of all men, the most fitted for seeing exactly the position which Gay Lussac's law would take, and for extending our view of it; but it was not so. His mind had probably become too much engrossed with his own views of the case; and the belief that the whole subject had been attained, came in at last to shorten his vision. It is not in science only that men show that they are not the best fitted for seeing their own position, and without this knowledge a false step is not easily prevented.

His memoirs, after this period, were generally on less important subjects. We see in them the same quality of originality, the same inclination to strike at the root of a subject, but it is done with less power; the result is rather an idea which he leaves to be worked out. Some of them are hurried, some are careless, some are unfinished. Had it lain within the scope of this memoir, I might have shown many sentences full of latent beauty, which have since budded and blossomed.

Of these the following is not the least remarkable, although it had gone farther than a mere idea; it was long put into practice by him. As far as I know, therefore, he is to be considered as the originator of analysis by volume, which has long been practised to a great extent in the manufactories of Lancashire. In this respect chemists are still behind Dalton, and have not yet got into the habit of using all those advantages which his works have offered, although the actual knowledge on the subject has advanced far beyond the period of Dalton's latest years.

In one of these memoirs on the analysis of spring and mineral waters, we find that he gives directions for the centigrade method of testing, and Mr. John Gough Watson, his pupil, informs me that he used it constantly. He says,*

* Mem. Phil. Soc., Vol. III., new series, p. 59, 1814.

"The improvements I would propose in the use of tests are, that the exact quantities of the ingredients in each test should be previously ascertained and marked on the label of the bottle; this might easily be done in most of them in the present state of chemical science. We should then drop in certain quantities of each from a dropping tube graduated into grains till the required effect was produced; then from the quantity of the test required, the quantity of saline matter in the water might be determined without the trouble of collecting the precipitate; or if this was done, the one method might be a check upon the other." This method of testing, which promises to be of such great value in saving the time of chemists, was then clearly seen by him, although it has taken several workers in the field to bring it into use in the laboratory, chemists, like others, being difficult to move into a new train of thinking and acting. At the same time the mere advice is not enough, it is needful to show how it may be accomplished in various instances; this, Dalton did partially. He gave the right directions as a master, leaving it for a long train of workmen to carry out his ideas. Still we see clearly that he was accustomed to use the graduated dropping tube, and analyze by volume. He gives directions for taking the alkalinity of water by the use of acids, and adds, that "these acids may be considered as sufficient for tests of the quantity of lime in such waters, and nothing more is required than to mark the quantity of acid necessary to neutralize the lime." Here we see that he was accustomed to take the alkalinity of waters for the carbonate of lime.

There is certainly a change in the style of these memoirs, there is less care, there are opinions thrown out and unfinished experiments which do not directly lead to benefit, and there is a diminished desire to give the ultimate laws on which phenomena depend. The mind had evidently felt that something had been achieved, which left it leisure and gave

it also a right to be heard, even when it uttered only its suspicions. In these memoirs we find prominently brought forward that intense faith in his own previous results, constantly quoting what was obtained in his own mind in preference to the results obtained by the whole world besides. This gradually led to a certain amount of egotism, and a conservative belief that all work in that direction was completely finished, so that he does not seem to look forward sufficiently to any improvement or modification.

Instead of reviewing his later writings, I shall add here a list more complete than has been yet given, although all the papers were not viewed by him as important, and some were merely given in all probability to supply an occasional want of material at the meetings of the Literary and Philosophical Society; by a perusal of the titles some idea will be given of his great fertility and diligence. These titles are taken from the books of the Society. On reading them over, one is compelled to wonder at the newness and youth of much of our modern science, and to doubt, on that account, the stability of not a few of its maxims; for we find that although it is intended to represent the thoughts of nature, it is of itself not older than "a man that shall die." After Dalton read his first paper, Mr. Robert Owen read one on March 6th, 1795, entitled "Thoughts on the connection between universal happiness and practical mechanics;" and in 1797, "On the origin of opinion with a view to the improvement of the social virtues." Mr. Owen is still alive.

LIST OF DALTON'S PAPERS.

Read before the Members of the Manchester Literary and Philosophical Society.

1. October 31st, 1794. Extraordinary Facts relating to the Vision of Colors, with Observations.

2. November 27th, 1795. On the Color of the Sky, and the relation between Solar Light and that derived from Com-

bustion; with Observations on Mr. Delaval's Theory of Colors.

3. April 7th, 1798. Essay on the Mind, its Ideas, and Affections; with an Application of Principles to explain the Economy of Language.

4. March 1st, 1799. A paper, containing Experiments and Observations, to determine whether the quantity of Rain and Dew is equal to the quantity of Water carried off by the rivers and raised by evaporation; with an Inquiry into the Origin of Springs.

5. April 12th, 1799. Experiments and Observations on the Power which Fluids possess of conducting Heat; with Reference to Count Rumford's seventh essay.

6. June 7th, 1799. On the Color of the Sky, and the relation betwixt Solar Light and that derived from Combustion; with Observations on Mr. Delaval's Theory.

7. April 18th, 1800. Experimental essays, to determine the Expansion of Gases by Heat, and the *maximum* of Steam or Aqueous Vapour, which any Gas of a given temperature can admit of; with observations on the common and improved Steam Engines.

8. June 27th, 1800. On the Heat and Cold produced by the Mechanical Condensation and Rarefaction of Air.

9. October 17th, 1800. Philological Inquiry into the Use and Signification of the Auxiliary Verbs and Participles of the English Language.

10. December 12th, 1800. Review of Dr. Herschel's Experiments on the *radiant* Heat, and the Reflectibility and Refrangibility of Light.

11. July 31st, 1801. Read Part 1st of Mr. Dalton's paper on the Constitution of Mixed Gases, &c.

12. October 2nd, 1801. Read Part 2nd of Mr. Dalton's paper on the Force of Steam, &c.

13. October 16th, 1801. Read Part 3rd of Mr. Dalton's paper on Evaporation, &c.

14. January 22nd, 1802. On the General Causes, Force, and Velocity of Winds; with remarks on the Seasons most liable to high winds.

15. October 29th, 1802. On the Proportion of the several Gases or Elastic Fluids, constituting the Atmosphere; with an Inquiry into the Circumstances which distinguish the *Chymical* and *Mechanical* Absorption of Gases by Liquids.

16. January 14th, 1803. On the Spontaneous Intercourse of different Elastic Fluids, in confined circumstances.

17. October 7th, 1803. On the Absorption of Gases by Water.

18. November 4th, 1803. On the Law of Expansion of Elastic Fluids, Liquids, and Vapours.

19. February 24th, 1804. A Review and Illustration of some Principles in Mr. Dalton's course of Lectures on Natural Philosophy, at the Royal Institution, in January, 1804.

20. August 3rd, 1804. On the Elements of Chemical Philosophy.

21. October 5th, 1804. On Heat.

22. November 30th, 1804. Review of Dr. Hope's paper "On the Contraction of Water by Heat."

23. September 2nd, 1805. Remarks on Mr. Gough's two Essays on Mixed Gases, and on Mr. Schmidt's, "On Moist Air."

24. March 7th, 1806. On Respiration and Animal Heat.

25. February 6th, 1807. On the Constitution and Properties of Sulphuric Acid.

26. October 2nd, 1807. On Heat.

27. October 16th, 1807. On the Expansion of Bodies by Heat.

28. January 22nd, 1808. On the Specific Heat of Bodies.

29. March 18th, 1808. On the Specific Heat of Gaseous Bodies.

30. December 2nd, 1808. On the Measure of Mechanical Force.

31. December 16th, 1808. On Respiration.

32. March 10th, 1809. On Evaporation.

33. April 7th, 1809. On the Compounds of Sulphur.

34. November 3rd, 1809. On Muriatic Acid.

35. December 1st, 1809. On Sulphuric Acid.

36. March 9th, 1810. On Fog.

37. November 16th, 1810. Appendix to his Remarks on Respiration and Animal Heat.

38. December 28th, 1810. On Hygrometry.

39. April 3rd, 1812. On Meteorology.

40. April 17th, 1812. Meteorology continued.

41. October 2nd, 1812. On the Oxymuriate of Lime.

42. January 8th, 1813. Experiments on Phosphoric Acid and the Phosphates.

43. March 5th, 1813. Experiments and Observations on the different Compounds of Carbonic Acid and Ammonia.

44. October 1st, 1813. On the Combinations of Gold.

45. October 15th, 1813. Continuation of the paper on the Combinations of Gold.

46. November 12th, 1813. The Combinations of Platina.

47. December 10th, 1813. On the Cause of Chemical Proportion, being remarks on a paper by Berzelius.

48. January 7th, 1814. Experiments on certain frigorific Mixtures.

49. March 18th, 1814. Remarks tending to facilitate the Analysis of Spring and Mineral Waters.

50. October 7th, 1814. On Metallic Oxides.

51. December 2nd, 1814. On Metallic Oxides. (Continued.)

52. January 27th, 1815. Critical remarks on some modern Chemical Phrases.

53. November 17th, 1815. Remarks on Saussure's Essay on the Absorption of Gases by Liquids.

54. October 4th, 1816. On the chemical compounds of Azote and Oxygen.

55. December 13th, 1816. An Appendix to the Essay on chemical compounds of Azote and Oxygen.

56. October 3rd, 1817. On Phosphurets, or the combinations of Phosphorus with Earths, Alkalies, Metals, &c.

57. November 21st, 1817. Observations on Oxides and Sulphurets.

58. November 13th, 1818. Observations on the Quantity of Rain during the last Twenty-five Years; with Remarks on the Theory of Rain.

59. December 11th, 1818. Summary of Observations on the Barometer and Thermometer, made at Manchester for the last 25 years.

60. January 8th, 1819. Experiments on the Force of the Vapour of Ether, to show the fallacy of some of Dr. Ure's Statements just published in the Philosophical Transactions.

61. April 16th, 1819. On Sulphuric Ether.

62. October 15th, 1819. On Alloys, particularly those of Copper and Zinc, and Copper and Tin.

63. November 12th, 1819. On Amalgams, and other Metallic Alloys.

64. December 10th, 1819. A Chemical Analysis of the Mineral Waters of Buxton.

65. October 6th, 1820. On Oil, and the Gases obtained from it by heat.

66. December 1st, 1820. On Alum.

67. January 26th, 1821. On Meteorology, or observations on the Weather for the years 1819 and 1820, in Manchester.

68. February 9th, 1821. Observations on Meteorology, particularly with regard to the Dew point, &c., or quantity of Vapour in the Air.

69. October 5th, 1821. Some observations on the Salts and Sulphurets of Iron.

70. November 30th, 1821. On the Effects of continued Electrification on compound and mixed Gases.

71. December 13th, 1822. On the Saline Impregnations

of the Rain which fell during the late Storm, viz., December 5th, 1822.

72. March 21st, 1823. Appendix to an Essay on Salt Rain (read December 13th, 1822), with additional observations on the succeeding Storms of Wind and Rain.

73. November 14th, 1823. On the Nature and Properties of Indigo; with directions for the valuation of different samples.

74. December 26th, 1823. On various Alloys of Tin, Zinc, Lead, Bismuth, Antimony, &c.

75. October 15th, 1824. On Associations for the Promotion of the Physical Sciences, Literature, and the Arts.

76. November 12th, 1824. An Account of some Experiments to determine the Light and Heat given out by the combustion of different Gases.

77. April 15th, 1825. Results of Meteorological Observations at Manchester, for Thirty-one Years; with Remarks upon them.

78. December 30th, 1825. On the Constitution of the Atmosphere.

79. October 6th, 1826. On the Height of the Aurora Borealis above the surface of the Earth, particularly the one seen on the 29th of March, 1826.

80. November 4th, 1826. An Appendix to a paper read on October 6th, on the height of the Aurora Borealis above the surface of the earth.

81. November 26th, 1827. An Historical Sketch of the Society's Library; with an Account of its present state.

82. December 28th, 1827. Observations, chiefly chemical, on the nature of the Rock Strata in Manchester and its vicinity.

83. October 17th, 1828. Summary of the Rain, &c., at Geneva and at the elevated station of St. Bernard, for a series of years, from the 'Bibliotheque Universelle' for March, 1828; with Observations on the same.

84. January 8th, 1830. Physiological Investigations, de-

duced from the Mechanical Effects arising from Atmospherical Pressure on the Animal Frame.

85. January 22nd, 1830. Remarks on a Statement of the Amount of Rain fallen at different places on the line of the Rochdale Canal.

86. March 5th, 1830. On the Quantity of Food taken by a person in health, compared with the Quantity of the different Secretions during the same period; with Chemical Remarks on the several Articles.

87. October 15th, 1830. Chemical Observations on certain Atomic Weights, as adopted by different Authors; with some Remarks on the Notation of Berzelius.

88. October 29th, 1830. Observations on the Causes of Colouring Matter.

89. November 23rd, 1830. Chemical Observations on certain Atomic Weights, as adopted by different Authors; with Remarks on the Notation of Berzelius.

90. January 21st, 1831. Meteorological Observations for a period of thirty-seven years; with Theoretical Remarks.

91. February 18th, 1831. On the Quantity of Oxygen in Atmospheric Air.

92. December 2nd, 1831. On the Proportion of Oxygen Gas in the Atmosphere.

93. January 13th, 1832. A Summary of Meteorological Observations, for 1831, made in Manchester and the vicinity.

94. January 11th, 1833. Dr. Dalton's Remarks on the Meteorology of the last year.

95. March 8th, 1833. Observations on the Anomalous Vision of Colours.

96. November 1st, 1833. A Description of an *imaginary* Aurora Borealis in the North of England.

97. February 7th, 1834. An Account of Meteorological Observations, at Manchester and other places, in the year 1833.

98. March 7th, 1834. Some Remarks on Clouds: their Nature, Height, &c.

99. October 17th, 1834. Observations on certain Liquids obtained from Caoutchouc by Distillation.

100. December 26th, 1834. Observations on the various accounts of the Luminous Arch or Meteor accompanying the Aurora Borealis of November 3rd, 1834.

101. February 20th, 1835. Account of Meteorological Observations, made in Manchester and other places, in 1834.

102. October 2nd, 1835. Read a paper by Dr. Dalton. (Subject not named in the Journal.)

103. February 15th, 1836. An Account of Meteorological Observations, made in Manchester and other places in 1835.

104. October 21st, 1836. Sequel to an Essay on the Constitution of the Atmosphere; read to the Society in the year 1825. Part 1.

105. November 4th, 1836. 2nd Part of a paper entitled "Sequel to an Essay on the Constitution of the Atmosphere."

106. October 2nd, 1838. On Arseniates and Phosphates.

107. February 5th, 1839. Some Account of Meteorological Observations, made in Manchester, in the years 1836-37-38.

108. October 1st, 1839. On the Ammoniaco-Magnesian Phosphate, as it was formerly called; or the Tribasic Phosphates of Magnesia and Ammonia, as Professor Graham has called it. And on the Phosphate of Soda and Ammonia, or Microscopic Salt, as it was formerly called; and now Tribasic Phosphate of Soda and Ammonia and Water, of Professor Graham.

109. March 31st, 1840. On the Quantity of Acids, Bases, and Water in the different varieties of Salts; with a New Method of Measuring the Water of Crystallization.

110. April 28th, 1840. Some Account of Meteorological Observations, made in Manchester, in the year 1839.

111. October 6th, 1840. Continuation of a paper On the Quantity of Acids, Bases, and Water in the different varieties of Salts.

112. January 12th, 1841. Meteorological Observations, made in Manchester and the neighbourhood, during the year 1840, or previously.

113. March 9th, 1841. On a New and Easy Method of Analyzing Sugar.

114. October 5th, 1841. On the Citric Acid, the Oxalic Acid, the Acetic Acid, and the Tartaric Acid.

115. January 10th, 1843. Meteorological Observations, at Manchester, made in the year 1842.

116. April 16th, 1844. On the Fall of Rain, &c., &c., in Manchester, during a period of 50 years.

Some of these were embodied in other works or printed elsewhere.

In Nicholson's Journal.

New Theory of the Constitution of Mixed Gases elucidated. Vol. III., p. 267. November 18th, 1802.

Letter from Mr. Dalton, containing Observations concerning the Determination of the Zero of Heat, the Thermometrical Gradation, and the Law by which dense or non-elastic Fluids expand by Heat. Vol. V., p. 34. April 20th, 1803.

Correction of a mistake in Dr. Kirwan's Essay on the State of Vapour in the Atmosphere. Vol. VI., p. 118. August 22nd, 1803.

On the supposed Chemical Affinity of the Elements of Common Air; with Remarks on Dr. Thomson's Observations on that subject. Vol. VIII., p. 145. June 16th, 1804.

Observations on Mr. Gough's Strictures on the Doctrine of Mixed Gases, &c. Vol. IX., p. 89. September 8th, 1804.

Facts tending to Decide the Question at what Point of Temperature Water possesses the greatest Density. Vol. X., p. 93. January 10th, 1804.

Extract of a Letter from Mr. J. Dalton: On a remarkable Aurora Borealis. Vol. X., p. 303. March 12th, 1805.

Remarks on Count Rumford's experiments relating to the maximum density of water. Vol. XII., p. 28. August 17th, 1805.

Investigation of the Temperature at which Water is of the greatest Density, from the Experiments of Dr. Hope, on the Contraction of Water by Heat at low temperatures. Vol. XIII., p. 377. April 14th, 1806. And Vol. XIV., p. 128. May 12th, 1806.

Inquiries concerning the signification of the word particle, as used by modern chemical writers, as well as concerning some other terms and phrases. Vol. XXVIII., p. 81. December 19th, 1811.

Remarks on Potassium, Sodium, &c. Vol. XXIX., p. 129. May 11th, 1811.

Observations on Dr. Bostock's Review of the Atomic Principles of Chemistry. Vol. XXIX., p. 143. May 15th, 1811.

In Thomson's "Annals of Philosophy."

Further Observations and Experiments on the Combinations of Oxymuriatic Acid with Lime. Vol. II., p. 6. 1813.

Remarks on the Essay of Dr. Berzelius, on the Cause of Chemical Proportion. December 24th, 1813. Vol. III., p. 174. 1814.

Vindication of the Theory of the Absorption of Gases by Water, against the conclusions of Saussure. Vol. VII., p. 215. 1816.

On the Chemical Compounds of Azote and Oxygen, and on Ammonia. Vols. IX., p. 186, and X., p. 38 and 83. 1817.

On Phosphuretted Hydrogen. Vol. XI., p. 7. 1818.

On the Combustion of Alcohol, by the lamp without flame. Vol. XII, p. 245. 1818.

On the Vis Viva. Vol. XII., p. 444. 1818.

In Phillips's "Annals of Philosophy."

On the Analysis of Atmospheric Air by Hydrogen. Vol. X. N. S.

In the "Philosophical Transactions."

On the Constitution of the Atmosphere. 1826.

On the Height of the Aurora Borealis. 1828.

Sequel to an Essay on the Constitution of the Atmosphere; with some Account of the Sulphurets of Lime. 1837.

In the "Annales de Chimie."

Sur l' Hydrogène Phosphuré. (Extract of a Letter addressed to the Royal Academy of Sciences.) Vol. VII. 1817.

In a Separate Form. 1840.

Essay on the Phosphates and Arseniates.

On Microcosmic Salt.

On the Mixture of Sulphate of Magnesia and the Biphosphate of Soda.

Essay on the Quantity of Acids, Bases, and Water in the different varieties of Salts; with a New Method of Measuring the Water of Crystallization as well as the Acids and Bases.

On a New and Easy Method of Analyzing Sugar.

His "Meteorology," 1793.

"Grammar," 1801.

"New System of Chemistry," Part 1, 1808—Part 2, 1810; Vol. II., Part 1, 1827.

A new edition of Vol. I., Part 1, appeared in 1842, and a new edition of his Meteorology in 1834.

His many letters to periodicals in his youth need not be specially enumerated.

These labours did not pass unnoticed in this country, and still less, I may say, in foreign countries. In 1816, the French Academy elected him a Corresponding Member,

although not yet a Member of the Royal Society of his own country.

Productions so numerous as these will account for all the years that Dalton had to spend. We find him still gaining his living as professional chemist, lecturer, and teacher of chemistry and mathematics; sometimes giving evidence on subjects connected with the arts, at trials in various courts; sometimes answering the inquiries of manufacturers. His uncertain position in this respect, suggested to Sir H. Davy that he might probably find it agreeable to undertake the scientific department of an expedition, fitted out by the Admiralty, for investigation in the Polar regions, under Sir John Ross. In 1818, Davy wrote to inquire if he would undertake it, but Dalton excused himself, partly on account of the hardships connected with a journey which, to one accustomed to a sedentary life, would be found too severe; and partly because he did not wish to interrupt his chemical inquiries. It is almost to be regretted that he did not undertake some such expedition. A new direction would have been given to his investigations; and a mind which had almost expended itself on its own field of view, would have had an ample new field on which new crops would, in all probability, have flourished abundantly.

About the same time, Mr. Strutt, of Derby, offered him a laboratory and a home at his house, with a salary of four hundred pounds a-year, and perfect freedom to spend his time in any manner he might consider agreeable. This he declined, on account of the loss of independence which he considered it would necessarily involve, although the offer was made on such terms as to free him from all duties whatever. This information I received from Alderman Shuttleworth, of Manchester, through whom the offer was made. But no professorship was offered him, none of the rich endowments of the country benefited Dalton. With a strange apathy these are seldom offered but to those who propose themselves;

there is no seeking out of talent, on which to confer opportunities of usefulness; and reward, which is an object less desired by men of elevated minds, comes only after the clamorous entreaties of friends.

The Royal Society had paid no attention to him, because constituted like many of the other public bodies of the country, receiving its strength by the vigour of individuals moving towards it, and so standing more in the condition of a reservoir than of a fountain. Davy proposed, in 1810, to nominate him, but he declined; probably the expense was the hindrance. There certainly are cases in which such a hindrance should not be permitted. In 1822, he was proposed and elected without his consent being asked, lest a true *nolo episcopari* should have again been uttered. It was in the summer of the same year that he visited Paris, and there had that distinguished reception and entertainment among its scientific men, that he might have looked for here in vain. His reception in Paris pleased him much; he returned much refreshed and invigorated in mind; he formed high opinions of many of the celebrated characters he met there, and the visit was spoken of with pleasure to the end of his life.

If the customs or laws of the Royal Society prevented them from early electing him, they took the first opportunity in their power of presenting him with the gold medal. In the discourse* of Sir H. Davy, on that occasion, we have his matured opinion on Dalton's position and discoveries. He there says that the medal was given "for the development of the chemical theory of definite proportions, usually called the atomic theory, and for his various other labours and discoveries in physical and chemical science." His speech at this time must be taken as his matured opinion on the subject of the atomic theory, whatever he may have in private said or been reported to say. It continues: "To Mr. Dalton

* Page 125, "Six discourses delivered before the Royal Society," by Sir H. Davy.

belongs the distinction of first unequivocally calling the attention of philosophers to this important subject. Finding that in certain compounds of gaseous bodies, the same elements always combined in the same proportion; and that when there was more than one combination, the quantity of the elements always had a constant relation, such as 1 to 2 or 1 to 3, or to 4, he explained this fact on the Newtonian doctrine of indivisible atoms, and contended that, the relative weight of one atom to that of any other atom being known, its proportions or weight in all its combinations might be ascertained; thus making the statics of chemistry depend upon simple questions in subtraction or multiplication, and enabling the student to deduce an immense number of facts from a few well authenticated, accurate, experimental results." He then mentions Bryan and William Higgins and Richter, saying, that "it is difficult not to allow the merits of prior conception as well as of very ingenious illustration to" W. Higgins. He expresses Richter's views very neatly, saying, "In his *New Foundations of Chemistry*, published in 1795, he has shewn that when neutro-saline bodies in general undergo mutual decomposition, there is no excess of alkali, earth, or acid; and he concludes, that these bodies are invariable in their relation to quantity, and that they may be expressed by numbers." He, however, continues to say,* "Mr. Dalton's permanent reputation will rest upon his having discovered a simple principle universally applicable to the facts of chemistry, in fixing the proportions in which bodies combine, and thus laying the foundation for future labours respecting the sublime and transcendental parts of the science of corpuscular motion. His merits in this respect resemble those of Kepler in astronomy.—Mr. Dalton has been labouring for more than a quarter of a century with the most disinterested views. With the greatest modesty and simplicity of character he has re-

* Page 129.

mained in the obscurity of the country, neither asking for approbation nor offering himself as an object of applause. He is but lately become a fellow of this society, and the only communication he has given to you is one, compared with his other works, of comparatively small interest; their (*i.e.* the council's) feeling on the subject is therefore pure. I am sure he will be gratified by this mark of your approbation of his long and painful labours. It will give a lustre to his character which it fully deserves; it will anticipate that opinion which posterity must form of his discoveries; and it may make his example more exciting to others in their search after useful knowledge and true glory."

Soon after this, in 1827, the first part of the second volume of his new system of chemistry was published, and we there see how the science would have grown had it been under the hand of Dalton alone. A vigorous hand he certainly had, but there were hundreds eagerly giving their attention to chemistry over all Europe, for the love of knowledge, of fame, and of humanity, so that it was driven impetuously forward, and under the direction of his own theory, had entirely left him behind; he was outrun by his own disciples; left alone by those who without him would not readily have moved.

In this volume he gives an account of the theory of the elementary nature of chlorine, but evidently without full belief, retracts gracefully his somewhat hazardous opinion that fluorine was a higher oxide of hydrogen than water, and uses the expression "oxide of potassium and sodium," without much disinclination. He was therefore willing to change his opinion, but in reality he had been so many years working to collect material for his book, that this volume was left behind before it was published. His dislike to much reading was found to be an injury to him, and many years of his life were thus found to be of little value to science. We must, therefore, in his public capacity view him now chiefly as one whose work was finished and who was enjoying the fruits of his labours,

although at the same time he seems to have worked as constantly as ever, retaining the original habits but not displaying the original vigor. To work too long in one vein seems to be highly injurious, as the vein narrows the labour increases in greater proportion than the yield, and it is better for us to return to the main centre of the mine, leaving the distant veins to be worked out when the approaches shall have gradually become easier.

In 1830, the Academy of Sciences at Paris elected him foreign associate, in the place of Davy. Of this addition to his scientific rank in that society, he was unreservedly proud. As if his own country were constantly to be behind in his recognition, we find that in the year 1832, Oxford elected him doctor of civil laws. This title of D.C.L., given at the recommendation of Dr. Daubeny, was one which he seldom omitted after his name; the simplicity of his mind did not allow him at all to disguise the pleasure with which it was viewed.

Being now advanced in years, his friends were anxious to secure for him an age less laborious than his life had been. With small means, he had still saved some money, but too little for the support of his declining strength during the years of his probable life. We are told by Dr. Henry, that Mr. Babbage first suggested the propriety of applying to government for a pension, whilst Mr. Geo. W. Wood and Mr. Poulett Thomson were most active in obtaining it.

Dr. Henry informs us that the first answer the Lord Chancellor gave was, that he "was anxious to obtain some provision for him, but that it was attended with great difficulty." This is an expression at which we cannot be too much astonished, but there is no doubt of its truth, even if it did not sound highly probable. Were there so many superior men pressing on the treasures of the country that it was difficult to obtain a pittance for Dalton? The same defect again shows itself, there is no spontaneous movement, but every man must thrust himself

or be thrust forward by his friends, and after having done his work by many struggles, another struggle must be made for the reward.

The following letter, written by Dr. William Henry, his intimate friend the celebrated chemist, was sent along with the formal application to government. Its beauty and truth demand for it a place in every memoir of Dalton.

DR. HENRY TO C. BABBAGE.

"Mr. Dalton never had, nor was ever given to expect any reward or encouragement whatsoever from government, and having been in habits of unreserved communication with him for more than thirty years, I can safely aver that it never occurred to him to seek it. He has looked for his reward to purer and nobler sources; to a love of science for its own sake; to the tranquil enjoyments derived from the exercise of his faculties, in the way most congenial to his tastes and habits, and to the occasional gleams of more lively pleasure, which have broken in upon his mind, when led to the discovery of new facts, or the deduction of important general laws. By the moderation of his wants and the habitual control over his desires, he has been preserved from worldly disappointments, and by the calmness of his temper and the liberality of his views, he has escaped those irritations that too often beset men who are over anxious for the possession of fame, and are impatient to grasp prematurely the benefits of its award. For a long series of years he bore neglect and sometimes even contumely, with the dignity of a philosopher, who though free from anything like vanity or arrogance, yet knows his own strength, estimates correctly his own achievements, and leaves to the world, generally although sometimes slowly just, the final adjudication of his fame. Among the numerous honours that have since been conferred on him by the best judges of scientific merit in this and other countries, not one has been sought by him. They have been without exception spon-

taneous offerings, prompted by a warm and generous approbation of his philosophical labours, and by the desire to cheer him onward in the same prosperous career. Deeply as he has felt these distinctions, they have never carried him beyond that sober and well regulated love of reputation, which exists in the purest minds, and is one of the noblest principles of action.

"In perfect consistency with Mr. Dalton's intellectual qualities are the moral features of his character, the disinterestedness, the independence, the truthfulness, and the integrity which through life have uniformly marked his conduct towards others. He has been taxed with plagiarism, but never was a charge more completely unfounded. Not only is he incapable of encroaching on the just rights of others, but even of taking tacitly to himself applause to which he does not feel that he is fully entitled. Of the work from which he is accused of having borrowed the outline of his atomic theory he had never once heard, until many years after the publication of his opinion on that subject. Nor is this at all extraordinary when it is considered that men like Mr. Dalton, of original and creative minds, trust rather to their own powers of research than to reading; and in the knowledge of the history of science are often surpassed by very inferior persons. This general remark applies to Mr. Dalton; but he is a discoverer in the true sense of the word. He has drawn from observed phenomena new and original views—upon these views he has founded distinct conceptions of a general law of nature;—he has traced out the conformity of that law with an extensive class of facts, many of which he himself first revealed by well-devised experiments, and he has thus secured an admiration not by having broached ingenious opinions merely, but by having worked out the evidences of those opinions by labours most sagaciously and perseveringly applied. Nor is it on the atomic theory only that his reputation must rest. It has a broader basis in his beautiful and successful investigations

into the subject of heat; into the relations of air and moisture to each other; and into a variety of other topics intimately connected with the stability and advancement of chemical philosophy. I therefore agree with you that Mr. Dalton has strong claims upon the national respect and gratitude, and contend for his title to reward, even though he may not have accomplished anything bearing strictly upon the improvement of those arts and manufactures, which are the chief sources of our national wealth. For let it be remembered, that every new truth in *science* has a natural and necessary tendency to furnish, if not immediately, yet at some future time, valuable rules in *art*. Nothing is more common than that a general principle, when first developed, may admit of no obvious practical use; but that a few subsequent discoveries, made perhaps at a small expense of genius or labour, supply links which render it available first to individual, and in due course to public wealth and prosperity. Not to mention other instances, Mr. Watt derived from Dr. Black's discovery of latent heat, a guiding light to the noblest invention that has ever been placed in the hands of man, for giving him control over the physical world, and even for advancing his progress in moral and intellectual cultivation. The discovery of chlorine also in the laboratory of a retired chemist, brought forth no practical results for several years, but when found by a subsequent philosopher to quicken the whitening of unbleached cotton and linen goods, it was immediately applied by practical men to the art of bleaching, and no one can now calculate its immense influence in giving rapid circulation to the capital employed in the cotton and linen manufactures. Among the abstract truths unfolded by Mr. Dalton, it would not be difficult to point out the germs of future improvements in the practical arts generally, germs which now lie dormant in the shape of purely scientific propositions.

"But were it otherwise it would surely be unworthy of a

great nation to be governed in rewarding or encouraging genius by the narrow principle of a strict barter of advantages. With respect to great poets and great historians, no such parsimony has ever been exercised. They have been rewarded, and justly, for the contributions they have cast into the treasury of our *purely intellectual* wealth. And do not justice and policy equally demand that a philosopher of the very highest rank, one who has limited his worldly views to little more than the supply of his natural wants, and has devoted for more than forty years the energies of his powerful mind to enlarging the dominions of science, should be cherished and honoured by that country which receives by reflection the lustre of his well-earned fame? The most rigid advocate of retrenchment and economy cannot surely object to the moderate provision, which shall exempt such a man in his old age from the irksome drudgery of elementary teaching, and shall give him leisure to devote his yet vigorous faculties to reviewing, correcting, applying, and extending what he has already in great part accomplished. In one instance of recent date, a philosopher who has eminently distinguished himself in purely abstract science, has received the merited reward of a pension for life. It is most desirable then that the British government, by extending its justice to another not less illustrious, should be spared the deep reproach which otherwise assuredly awaits it, of having treated with coldness and neglect one who has contributed so much to raise his country high among intellectual nations, and to exalt the philosophical glory of the age."

The application met with success, and at the meeting of the British Association in Cambridge, it was announced by Professor Sedgwick, that the king had granted a pension of £150 to Dalton. This announcement, in the beautiful language of that eloquent man of science, has been frequently quoted, and is well known.

In 1836 the pension was increased to £300, but two years previously his brother Jonathan died, leaving for him as heir the paternal estate, which now made him comparatively wealthy. He, however, according to his strength, still continued working. So late as 1840 he published four essays, with the title "On the Phosphates and Arseniates; Microcosmic Salt; Acids, Bases, and Water; and a New and Easy Method of Analysing Sugar."

Here we have another instance of his old method of striking roughly in a new direction, and deciding at once on the whole district, little caring who was to come after to examine. He says, p. 10, "The new method of ascertaining the quantity of water in the salts is now to be discussed. I have a bottle with a stopper which just contains 572 grains of pure water, when the stopper is put on and wiped clean and dry, at the temperature of 60° Fahr. A graduated tube or jar is necessary, of 5in. or 6in. long and one quarter of an inch in diameter, to measure exactly to a grain of water. A platina wire is appended to the neck of the bottle, so as to be weighed more conveniently. An ounce, more or less, is to be weighed of any salt; it is then to be put into the bottle, capable of containing 572 grains of pure water (the water having been carefully tansferred into another glass vessel of more ample dimensions), and the salt dissolved and carefully transferred and weighed in the 572 bottle again, and the spare liquor, if any, is to be put into the narrow graduated tube.

"We have then 572 of pure water, + the pure water of the salt + the solid (or liquid water of the salt whatever it may be), all together in a liquid form, in the bottle and the narrow tube. I was greatly surprised at the results. If the salt was anhydrous, it would all go into the bottle exactly filling it to a grain; showing that the salt enters the *pores* of the water.

"If the salt contained water, the quantity of water was

measured by the narrow tube in all cases whatever, showing that the solid matter had in reality entered the pores of the water."

This principle he applied to the analysis of sugar, showing that its bulk in solution was equal to the amount of oxygen and hydrogen combined as water, the carbon not occupying any room. This rule Dalton considered as absolutely and universally true. He called it "the greatest discovery next to the atomic theory." This idea in the hands of Messrs. Playfair and Joule has had a fertile expansion, although Dalton's mode of expressing the law has been limited to certain classes of salts.* Had time and strength been given him, he would no doubt, after this commencement, have laboured well in the field of "atomic volume."

It is well when men become aware of the failure of their powers, and are willing to give up their places to those whose minds are in full vigor. The essay on the phosphates and arseniates affords, on page 12, a melancholy instance of the fate of those who overrate their strength. This sentence occurs in the form of an epitaph.—"I sent the account of the phosphates and arseniates to the Royal Society, for their insertion in the transactions. They were rejected. Cavendish, Davy, Wollaston, and Gilbert are no more." It sounds like an epitaph on himself, and the volume tells still more plainly that he had not followed the increased exactness required in science. Nevertheless, in one respect, the last pamphlet is a model of himself, the rapid, hasty work, the carelessness of the labours of others, and the new field struck out in his remarks on sugar. In one point, however, it fails; in his early life he did not work otherwise than on the most advanced ideas; amongst the phosphates and arseniates he had receded.

The position which Dalton had attained seemed to demand

* See "Memoirs of the Chemical Society." Vol. II., &c.

some public demonstration of honor in the town he had so long adorned, and his declining years suggested a permanent memorial. In 1834 his friends decided on having a marble statue, which should present a correct likeness, and for this purpose Chantrey was selected as the most suitable sculptor. Chantrey seems to have entered on the task with pleasure, and he has done it well. This statue is in the entrance hall of the Royal Institution, in Manchester; the trustees having charge of it on condition that no one shall be refused permission to look at it. Dr. Henry says the likeness is more ideal than the reality, a refinement being given to the countenance which did not exist in the bust which Chantrey first took and used as a model when engaged on the full figure. Dr. Henry's intimate knowledge of Dalton must prevent any one from entertaining a very different opinion, but a daguerreotype profile now before me taken from life,* shews not only the marked features of the thinker, which no one has denied as they were striking, but that peculiar refinement which gives the idea of the student and the gentleman. This small photograph on a silver plate, is exactly similar to the head so beautifully engraved by Stephenson, I suppose indeed that it served as the copy; every expression is the same, and every fold of the abundant white hair, nor can I see that the engraver has increased the refinement, although he has probably somewhat heightened the forehead.

In the same year, I believe, he was presented at court, a place that seemed scarcely to suit such a man, but he seems to have had no desire to evade any of his natural claims to honor, taking them as a necessary consequence of his work, neither too highly elated like the great majority who are honored, nor painfully retiring like Cavendish.

Being a Quaker, and not able to wear a sword, he was taken in the scarlet robes of an Oxford Doctor of Laws, and

* This belongs to Mr. John Parry, who assisted in taking it.

although it was feared that scarlet would scarcely suit one of the Society of *Friends*, Mr. Babbage, who took him, remarked that as he had a "kind of colour blindness, all red colours appeared to him of the colour of dirt." Mr. Babbage adds, "besides, I found that our friend entertained very reasonable views of such mere matters of form;" a remark perfectly true. Dalton was no bigot or formalist. The ceremony was rehearsed beforehand by his friends, and it passed over well, but not without remark on the length of time that he remained before the king, who detained him long enough to ask him several questions. Some say that Dalton, not imagining that he had to pass on without a word of conversation, had waited to be spoken to, and somewhat embarrassed his Majesty, in his desire to be civil, to find suitable questions to put to him. But Dalton had learnt his part well, and the reason of the honor that he had of staying a few seconds longer than anyone else, until people began to ask who he could be, is more likely to have been caused by the fact which Mr. Babbage mentions, that the king was informed of the unusual presentation by Lord Brougham, who was Lord Chancellor at the time, and nominally presented him.

Honors of various kinds soon became familiar to him, such as fellowships of societies and degrees from universities, of which the title of LL.D., from Edinburgh, was one. They came upon him as on one to whom they were welcome, but remained entirely external to him; his life had been complete without their aid, and it was too late for them to find a perfect sympathy either in his intellect or his habitual feeling.

It is to be regretted that Miss Johns has not preserved more about Dalton, as she had the ability and also his confidence; we might have obtained through her means a better picture of his mental and moral phases. In the year 1830, when the Johns family left the town, Dalton took a house close to their

old residence and from that time lived alone. In 1832, at the age of 66, he ascended Helvellyn as firmly as ever, took the ladies an excursion in Cumberland to see his old friends, playfully introducing the two elder Miss Johns as his daughters, and their cousins, the younger ladies, as his granddaughters. But we cannot follow him on the hills; of them he never seems to have wearied, nor did he ever weary of his old friends there, nor of the study which first made him look on the peculiar aspects of nature at that spot. The place seemed to be a memory both for his intellect and his heart, and his love for the district shows how permanent in him were the feelings of both.

It was not until 1837 that Dalton felt in any decided manner the progress of age, although long before that time his energy and originality had diminished. Properly speaking he had, like many other gifted persons, only one period of great originality, occurring immediately at the conclusion of his education, so to speak. At that period it frequently happens that the mind makes choice of the materials with which it will work, and has some more or less distinct idea of the conclusion, whilst the rest of the life is directed to its elucidation or expansion.

On April 18th, 1837, he was disabled for a while by paralysis, and although he wrote some memoirs after this period he never entirely recovered. He fell suddenly to the floor after his usual morning's labor of recording the state of the barometer and thermometer. His friend Peter Clare was his constant companion, and to the end of his life acted both as secretary and friend, with an amount of reverence and affection that is seldom found. He noted down the observations when Dalton was ill, and took down at his request all the minute particulars connected with his illness and his medicines; for every illness and every dose was like a chemical experiment with Dalton.

In six weeks he had recovered, but on February 15th, 1838, he was again attacked, and was left very much enfeebled. His habits were not changed; idleness did not follow on weakness; he still made his experiment, but took, as he said, four times longer to perform it; still saw his friends, and kept up the average of his cheerfulness, although the sad feeling that his frame had decayed, was not absent from his mind. Of this he gave little indication, but when the conversation happened once to turn upon an eminent scientific man whom he admired, and had seen in France, he said, "Ah! he was a wreck then as I am now."

Mr. Ransome, once his pupil, and latterly his friend and medical attendant, informs us that from 1838 he required constant attendance, although he had no other attack until near his death. During the 1843-4 session of the Literary and Philosophical Society, he attended occasionally, where since 1817 he had been president. He had then lost his strength so much, that to walk across the two intervening streets to his own house in Faulkner-street, leaning on the arm of Mr. Clare, was a great exertion. His speech was feeble and inarticulate, so that he did not attempt to address the society. On May 20th, 1844, another slight fit occurred. Still we find him at his work, feebly, but regularly putting down his metereological observations. His earliest thoughts were on science, and they endured to the latest period of his life. On July 26th his servant observed that his hand was unusually tremulous, but, as Dr. Wilson observes, "there was the same care and corrective watchfulness manifested in the last stroke of his pen." He had written down "little rain this," and observing after a while that he had left out "day," he returned to complete it. On the morning of the 27th, about six o'clock, his attendant left him for a little, enforcing on him the propriety of not moving until his return. His feeble limbs had not even then got reconciled to perfect helplessness; he seems to

have made an attempt to rise, but fell from the bed, and was found with his head on the floor, perfectly lifeless.

Mr. Harland, in the *Manchester Guardian* of the period, gives a copy of the last three lines he wrote: they will serve as an illustration of his method.

Day of Month.	Hour.	Ther. in.	Ther. out.	Barom.	Wind.	Strength.	Rain.	Evap.	Remarks.
July 26th.	8	65	75	30.03	S.W.	1			
——	13	65	73	30.10	Sy.	1			
——.	21	60	71	30.18	S.W.	1	Little	Rain	

He had ceased for some months to make observations on the amount of rain and evaporation. Mr. Harland reckons his total observations at 200,000.

His life ended with science, and these few of his observations are therefore not out of place even when recording that solemn moment. So calm had been his life, that it is not surprising that in death his countenance should show a "beautiful repose," as the same writer observed in a memoir fitted for a more permanent place than a newspaper.

Many would like to know something more of Dalton's religious faith, and would expect to learn from his concluding words the hope and direction of his spirit, as if from its position at that moment they were able to calculate the angle of its divergence from earth. But that spirit had ceased to find utterance for itself, and we are compelled to look at the more solid points of a laborious lifetime. Scientific men are often far from orthodox Christianity, although sometimes living like saints, lives of purity, charity, devotion, and deep reverence. Dalton "did justly," he "loved mercy," he "walked humbly," he remembered carefully, as his will especially shews, the mercy due to "the fatherless and widows," and all our accounts speak of him as one to all appearance in an unusual degree "unspotted from the world." His profession

was according to the spiritual, but inexpressive forms of the Society of Friends.

As is usually the case on the death of an eminent man, the first proof is furnished to many persons that he was once alive. It was suddenly known that a man of eminence had left us, and the greatest desire was shown to do his memory justice. Although the body of *Friends* to whom he belonged objected, the funeral was given up to be conducted by the authorities of the town. The remains, in a lead coffin enclosed in a solid oak one, were placed in the Town Hall, and for some days a constant flow of silent and gazing spectators passed through the building. Some objected to this form; but it is not easy to say what in such cases should be done. True honor can be given only by the mind and by the heart; but to honor any man publicly it is not enough that we feel it; it must be expressed. This was a solemn method of impressing it on all the forty thousand visitors, as well as on all the town and neighbourhood, who were aware of what was going on, and one probably which would leave a greater impression than a speech over the grave, heard only by a few. True, there was the explanation of his greatness wanted, but that can be given only to a few at a time, and in place of that there was the long continuation of the ceremony which, united with a full account of Dalton's life given in the *Manchester Guardian*, impressed the fact on many and enabled everyone to know why this man was so peculiarly selected for honour.

The funeral took place on the 12th of August. The train was nearly a mile in length, including most of the public bodies of the town, numerous private friends, and still more admirers on foot or in carriages. The town was occupied for a time with the burial of Dalton; the business ceased; the streets were thronged with numberless spectators; and the police of Manchester attended with a badge of mourning. The burial took place in the Ardwick Cemetery, on the south-east

side of the city. The grave is enclosed by a strong rail, enclosing a space about twenty feet square, and contains a very plain and simple, but large, massive, and imposing covering of polished red granite, with the inscription in large letters, JOHN DALTON, and in smaller letters the date of his birth and death.

This tombstone, consisting of a solid granite pediment and overhanging slab, was not made till some years after Dalton's death, when a subscription was raised, amounting to £5,312, in order to carry out some of his original intentions, as well as to connect his name with some public benefaction, as a most fitting memorial. Dalton had originally set aside two thousand pounds for a "professorship of chemistry at Oxford, for the advancement of that science by lectures, in which the atomic theory as propounded by me, together with the subsequent discoveries and elucidations thereof, shall be introduced and explained." The desire to repair the losses sustained by Mr. Johns; to show a mark of respect and gratitude to Mr. Peter Clare, who had been so affectionate as a friend; and to Mr. Neild, at whose table he had been welcomed regularly for many years, seems to have caused him to alter his will, and to attend rather to persons than to institutions.

His will included the names of many who were near and dear to him and needed assistance.

With great respect for the affection shown in Dalton's will to his friends and relatives, the sum mentioned was subscribed to carry out in part his original intention. Since his death Owens College had been founded in Manchester. For this there have been provided two Dalton chemical scholarships, of fifty pounds for two years; two Dalton mathematical scholarships, of fifty pounds, also continuing two years; Dalton prizes from ten to twenty-five pounds, and a Dalton natural history prize of fifteen pounds.* A thousand pounds of the

* As advertised for 1856.

money has been devoted to obtain a bronze statue, copied from Chantrey's marble one. This has been considerably enlarged, so as to suit its position at the right hand of the centre of the Infirmary, the widest public space in Manchester, and beside the statues of other distinguished persons.

Thus although in Manchester pure science is, from peculiar circumstances, allowed only a humble station beside the practical, and very few young men are allowed to study it thoroughly, sufficient energy and enthusiasm have been found to obtain for Dalton a memorial which connects his name as well with the ornament of the city, as with the hopes of all those youths whose aspirations lead them to seek eminence or usefulness in the study of the natural sciences.

CHAPTER XIII.

DALTON'S INTELLECTUAL CHARACTER.

THE question of greatest interest and importance connected with Dalton's personality relates to the character of his mind, not in a social point of view, for there we find that although the qualities were of the best material, they were not made prominent portions of his life, but intellectually in the faculty which caused him to place himself in history and connect his name with the knowledge of nature. Sir H. Davy has given him the highest character, when he said that "he was one of the most original philosophers of his time, and one of the most ingenious," and when he says that he "had none of the manners or ways of the world," and "was a very disinterested man." But in his sketch we do not see that respect with which a man having such a character ought to be treated. It is said for example, that "he had no ambition beyond that of being thought a great philosopher." Now this is a sneering expression, but, after all, is it not expressive of the whole aim of Davy's life? Still, at his noble ambition no one has ventured to sneer. Davy called it "glory," and united to his scientific discoveries fine poetic diction, but his love of nature was not so single as Dalton's, and although his sight was more delicate it was not so penetrating. There are few great men who have not had their peculiarities; when these arise from simplicity of character they have generally been considered to exalt their possessors. Davy's speculations on Dalton's course of thought are given at random. For example, he supposes him to have seen the works of the Higginses, but not Richter's,

whereas the contrary was the case, the works of the former not having been heard of, but some of the latter. We are not justified in making such speculations without some foundation. This "character" referred to, written by Davy, and to be seen in Dr. Henry's life of Dalton, might probably have been modified before he gave it to the public, had he lived to do so. He certainly had a right to jot down his own speculations in private.

Dalton has been called a coarse experimenter. He taught himself and never advanced with the times, but there are many varieties of gifts, and we have not always found that the finest experimenter has been the greatest discoverer. The mind in reality makes the experiment first. Experiments are not made on things distant from our knowledge, but on those which approach nearest to it; a theory is therefore formed, arising from previous knowledge, or a question is asked without a theory, exactly at the turning point where a finger post is for a moment wanted. The mind always travels the road or by-road of theory, although wavering at the meeting of new roads. Now Dalton when he saw that the road must be in a certain direction, did not care to keep by it at every step, and so surveyed a great extent of territory. It was done with the quick decision and instinct of the hunter over wild ground. One only laments that on the first sight of new lands there was not the poet to burst out into song. It is this want of poetry, this constant plodding workmanship of the intellect, that has obtained so few admirers for Dalton, and has allowed men, whose fame might readily be got from a very few of his memoirs, to take a position in science and society, which ought to have been far inferior. Even scientific men have yielded to the feeling, and, like the world of fashion, have admired the gayest. But, after all, how few are the scientific men whose diction gives life to their discoveries. Life is scarcely apparent till after much nursing.

The mind never ceases to be strange to us, and if our pictures of men are incorrect, it often arises from our desire to comprehend thoroughly the whole. This is always difficult, probably impossible, still more so in the case of one so little demonstrative. But it is not here desired to describe that substratum of mind common to all men, but those striking features which stood above the average man, and are the true source of our interest.

The first thing that we observe in Dalton is clearness of conception; he knows what he thinks, and can define it. This is very clear through all his course, every thought is squared and finished. To this more than anything else I attribute his first idea of atoms. He was obliged to conceive of gas, and how could he do it without giving shapes to the parts? Gases could not be without parts, they expanded and contracted, and so the parts became essential to them.

The next thing to observe is directness. He went to his point rapidly. His experiments are simple, and, although rude, are exceedingly appropriate. It must, however, be remembered that although simplicity is at times beautiful, it cannot be attained in experiment so easily now as then. He prepared the way for more complex methods. Great clearness of conception often leads the mind to put down its results in form and figure, giving a mathematical character to them. It loses the poetical distance by working at the foreground, but does not forget that the foreground has a beauty and truth of its own.

The third characteristic is tenacity. His conceptions once formed seemed never to fade, or were with difficulty eradicated. This is natural to a mind with strong conceptions. Its own thoughts become its material, much more than anything said or done by others, and it prefers to reason from its own data, being those best known to it. This was remarkably seen in Dalton.

Fourthly. I would add rapidity of conclusion. This may arise in various ways. In a mind with weak conceptions or pictures of things, rapidity may be great, but can be of no value. In a tenacious mind like Dalton's, rapidity of conception is a combination of true ideas, so rapidly made that the steps are not seen clearly by the mind itself, and hence that inexplicable result which seems at least one of the ways in which genius is produced.

To understand the truth of these remarks the memoirs must be read and compared with the times, for Dalton, although he died but lately, flourished at the birth of true chemistry, and his work was done when Berzelius was only commencing.

By him laws were more easily treated than facts, and thought was easier than observation. His mind was one of those which especially sought laws, desiring to form the link between the mental and material. He seldom observed without reasoning, and he had no surplus observation: this made him of an absent disposition. He seldom reasoned without observing: this made him an experimenter. But in the movements of his mind, as of his body, there was a certain rigidity, which he himself seems to have felt, and for which others have endeavoured to find causes. One quality Dalton had to a degree almost unparalleled: the constancy with which he clung to his occupation of observing and generalizing.

His mind seems to stand before us as an intellectual tool, constantly planing, drilling, and boring with never ceasing activity, without any violent fits of haste, and without any seasons of absolute rest. As far as I know I have indicated what were the peculiar characteristics of that tool, but there is no doubt that other circumstances might have brought into activity a far different set of faculties. We see prominently in him how one portion of the mind was willing to devote itself to obscurity for the advancement of the others, how the faculty which reasoned on the "Mind its ideas and affections,"

and upon the method by which thought is expressed, after a very few struggles, gave way to the more active faculties devoted to physical nature.

Some have objected to such characters by saying that they result from a want of full development of the faculties, from a one-sidedness of the mind; but what man in this short life can cultivate one faculty highly without depriving the others of nutriment, or cultivate every faculty equally without stinting them all?

In Dalton we have found a mind which has shewn itself, as well in its operations as in its results, to be of the very highest scientific class.

CHAPTER XIV.

ARRANGEMENT OF THEORIES.

To those who have attended to the history of ideas, it must always seem a remarkable thing that in early times, among certain truths, the very culminating point should have been sometimes attained by one grand bound over all the difficulties of the journey, leaving undescribed all the ruggedness of the ground passed over. To those who have been born on this summit, there is the same difficulty in passing down to the plain, as there is for us to pass upwards: the same blasting of rocks and filling up of morasses. To say that the history of the struggle is lost to us does not explain all. In these three words, already quoted, "measure, weight, and number," as applied to creation, we see that men had looked long and carefully on the world, had admired its beauty, and believed that everything was arranged with scrupulous accuracy. The context shows that the fundamental idea was a moral one, and the reasons we now have were not then in existence; but in general terms there is the eminence seen on which they stood; we move back towards the plain, or try to reach the summit, measuring, weighing, and numbering, and proving the great mountain view to be true to the minutest particle of matter.

The scientific mind seeks to repeat the order of thought by which the divine mind arranged the universe; the artistic mind sees it completed, and rejoices in its beauty. The scientific mind has the work of the six days of creation, the artistic mind lives in the Sabbath of rest. The poetical mind refuses this class of limitation.

These remarks are called forth by the peculiar progress of some of the early opinions and the difficulties encountered when experiment began. We may arrange the numerous theories spoken of in the following manner, seeking the spirit or fundamental idea of each.

First we have one matter out of which all the others were made. This in early times was real substantial matter, as water and earth; afterwards a dynamical water or earth, or a natural force corresponding to them or underlying them. Then the four elements were made the origin of all things; but convertible. These theories, although under some aspects going under various heads, may be conveniently put under one and be called the *Allhylic.* (ἄλλος and ὕλη, interchangeable elements.)

The next, although allied, claims for itself a separate place. When there is recognised a universal matter, a *prima materia* from which all things are made, and which itself has no substantial qualities, but is capable of assuming all, it may be called the *prothylic* theory. (πρώτη ὕλη, first element as it was called.)

When matter is made up of indivisible particles, the name *atomic* is already appropriately given. (α and τόμος, uncut.)

When particles are infinitely divisible, it may be useful to call this theory the *dia-tomic.* (διὰ and τομος cut through.)

When we find the original matter to be a force only, whether represented by a number, a point, a line, a geometrical figure, or a more abstract idea, this is the *dynamical* theory.

When there are neither forces nor atoms, nor distinct elements, nor universal and insuperable laws, nor a substratum of primary matter, the *mystic theory* seems an appropriate name.

In the early atomic theories, the only difference recognised in atoms is their shape. These theories *are mechanical.* Now we recognise many original differences forming elements. This is a *polyhylic* atomic theory.

2 P

No theory can entirely get rid of Dynamics, but it would only introduce confusion into the historical account, if we said more of it than the promoters themselves.

The old theories were after all exceedingly indefinite, notwithstanding the appearance of exactness. The Daltonian theory is remarkable chiefly for its idea of quantity. It defines composition and combination by quantity. It is mechanical, because it unites pieces of matter in a mason-like style; it fits every part and breaks none, but it is not merely mechanical. Force is required, and this is of a different kind with every species. It is polyhylic. It unites therefore more qualities than previous theories. There has never been any progress made in ascertaining in what essentially consists the peculiarity of the forces in each element. That remains for future inquiry. The inquiry has chiefly had relation to weight, and for that reason I have called Dalton's the quantitative atomic theory. Mr. Joule's discoveries in heat, although not purely chemical, have begun to introduce into chemistry, through physics, forces that can be exactly calculated other than weight.

I think it of importance that Dalton's theory as adopted by chemists should have a distinct adjective to express it. The term *quantitative atomic* connects itself with analysis which in every case leads us to the use of the theory, although the most convenient term for general use, by which also we do honor to the originator, is the *Daltonian* theory. It represents the mode of discovery by weighing and calculating, and the greatest fact treated of with regard to atoms, as viewed by chemists, that they are comparative quantities measurable by the balance: it represents also the state of chemistry since the theory was propounded, a wonderfully elaborate collection of orderly arrangements of bodies distinguished principally by their quantities. I have left out as unnecessary to this history such of the characters of atoms as Dalton held more as hypothetical than theoretical.

Those theories, which now appear only as histories, are not of necessity all extinct; for some of them a resurrection day will in all probability arrive, when their forms will be beautified and their powers exalted. It may be, too, that in the confusion of transition or the difficulties of progress, various inferior appearances may be looked on as the final triumph, and be hailed by the hasty and the short-sighted, although seen by the clearer judgment to be mere phantoms by the way, specimens of the mystic theory. The time may come when the old theory of the *prima materia*, which has deluded so many, may have a higher existence, and alchemy again become the "true philosophy." Over this, too, the dynamical theory may rise, grasping them all, and by giving clearer ideas of force, thrust into an inferior position both quantity and matter, and show us, with greater certainty, the true position of abstract power and of mind.

THE END.

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W. Fairbairn, F.R.S., Inst. Nat. Sc., Par. Corresp.	October 29th, 1824
W. A. Fairbairn	October 30th, 1849
David Gibson Fleming	January 25th, 1852
Richard Flint	October 31st, 1818
Benjamin Fothergill	January 23rd, 1855
H. R. Forrest	April 29th, 1856

	DATE OF ELECTION.
E. Frankland, Ph.D., F.R.S., Owens College	April 29th, 1851
Alfred Fryer	January 24th, 1854
Rev. William Gaskell, M.A.	January 21st, 1840
Samuel Giles	April 20th, 1836
John Gould	April 20th, 1847
John Graham	August 11th, 1837
Robert Hyde Greg, F.G.S.	January 24th, 1817
Robert Philips Greg	October 30th, 1849
John Clowes Grundy	January 25th, 1848
Richard Hampson	January 23rd, 1844
John Hawkshaw, F.R.S., F.G.S., and M. Inst. C.E.	January 22nd, 1839
William Charles Henry, M.D., F.R.S.	October 31st, 1828
John Hetherington	January 24th, 1854
Sir Benjamin Heywood, Bart., F.R.S.	January 27th, 1815
James Heywood, M.P., F.R.S., and G.S.	April 26th, 1833
James Higgin	April 29th, 1851
James Higgins	April 29th, 1845
Peter Higson	October 31st, 1848
John Hobson	January 22nd, 1839
E. Hodgkinson, F.R.S., M.R.I.A., F.G.S., &c.	January 21st, 1820
George Holcroft	January 24th, 1854
Isaac Holden	January 23rd, 1855
James Platt Holden	January 27th, 1846
Thomas Hopkins	January 18th, 1823
Henry Houldsworth	January 23rd, 1824
William Jennings Hoyle	February 7th, 1854
John Jesse, F.R.S., R.A.S., and L.S.	January 24th, 1823
Richard Johnson	April 30th, 1850
William Johnson	January 22nd, 1856
Rev. Henry Halford Jones, F.R.A.S.	January 22nd, 1856
Joseph Jordan	October 19th, 1821
B. St. J. B. Joule	April 18th, 1848
James Prescott Joule, F.R.S., &c.	January 25th, 1842
William Joynson	January 27th, 1846
Samuel Kay	January 24th, 1848
John Lawson Kennedy	January 27th, 1852
James Kershaw, jun.	January 24th, 1854
Richard Lane	April 26th, 1822

	DATE OF ELECTION.
William Langton	April 30th, 1830
Joseph Leese, jun.	April 30th, 1850
Thomas Taylor Lingard	January 22nd, 1856
Thomas Littler	January 27th, 1825
Joseph Lockett	October 29th, 1839
Benjamin Love	April 19th, 1842
Isaac Waithman Long, F.R.A.S.	January 27th, 1852
George Cliffe Lowe	January 24th, 1854
Edward Lund	April 30th, 1850
George T. Lund	January 23rd, 1855
William Tudor Mabley	October 30th, 1855
James M'Connel	October 30th, 1829
William M'Connel	April 17th, 1838
Alexander M'Dougall	April 30th, 1844
John Macfarlane	January 24th, 1823
Edward William Makinson, M.A.	October 20th, 1846
The Right Rev. the Lord Bishop of Manchester, D.D., F.R.S., F.G.S.	April 17th, 1849
Robert Manners Mann	January 27th, 1846
Thomas Mellor	January 25th, 1842
William Mellor	January 27th, 1837
John Moore, F.L.S.	January 27th, 1815
David Morris	January 23rd, 1849
Alfred Neild	January 25th, 1848
William Neild	April 26th, 1822
James Emanuel Nelson	January 27th, 1852
Thomas Henry Nevill	February 7th, 1854
John Ashton Nicholls, F.R.A.S.	January 21st, 1845
William Nicholson	January 26th, 1827
Edward Byron Noden	October 31st, 1854
Henry Mere Ormerod	April 30th, 1844
George Parr	April 30th, 1844
John Parry	April 26th, 1833
George Peel, M.Inst.C.E.	April 15th, 1841
Henry Davis Pochin	January 24th, 1854
John Ramsbottom	February 7th, 1854
Joseph Atkinson Ransome, F.R.C.S.	April 29th, 1836
Thomas Ransome	January 26th, 1847

	DATE OF ELECTION.
Richard Roberts, M.Inst.C.E.	January 18th, 1823
Samuel Robinson	January 25th, 1822
Alan Royle	January 25th, 1842
Samuel Salt	April 18th, 1848
Archibald Sandeman, M.A., Owens College	April 29th, 1851
Michael Satterthwaite, M.D	January 26th, 1847
Edward Schunck, Ph.D., F.R.S.	January 25th, 1842
Alexander J. Scott, M.A., Principal, Owens College	February 7th, 1854
Edmund Hamilton Sharp	January 23rd, 1855
John Shuttleworth	October 30th, 1835
Joseph Sidebotham	April 20th, 1852
George S. Fereday Smith, M.A., F.G.S.	January 26th, 1838
Robert Angus Smith, Ph.D., F.C.S.	April 29th, 1845
Peter Spence	April 29th, 1851
Thomas Standring	January 27th, 1852
Edward Stephens, M.D.	January 24th, 1834
James Stephens	April 20th, 1847
Robert Stuart	January 21st, 1814
John Edward Taylor	January 22nd, 1856
David Thom	April 20th, 1852
James Thompson	April 18th, 1854
James Aspinal Turner	April 29th, 1836
Thomas Turner, F.R.C.S.	April 19th, 1821
Absalom Watkin	January 24th, 1823
Joseph Whitworth	January 22nd, 1832
Samuel Walker Williamson	April 19th, 1853
William Crawford Williamson, F.R.S., Owens College	April 29th, 1851
George Bancroft Withington	January 21st, 1851
William Rayner Wood	January 22nd, 1839
Alonza B. Woodcock	October 30th, 1855
George Woodhead	April 21st, 1846
Edward Woods	April 30th, 1839
James Woolley	November 15th, 1842
Robert Worthington, F.R.A.S	April 28th, 1840

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OF THE

Literary and Philosophical Society of Manchester.

I.—*On Lightning and Lightning Conductors.*

By the late Mr. William Sturgeon.

(Continued from Vol. IX. p. 79.)

52. My own experience, which has been extensive in electro-explorations of fogs, corresponds with that of all other observers. I have found them invariably electro-positive; and in many instances, especially in frosty mornings, the well-known odoriferous character of electricity is so remarkably well developed on the olfactories, that no electrician could mistake it.

53. Haze, also, which occurs during hot weather, and floats higher in the air, and is more attenuated than fogs, I have found to be still more pregnant with intense electro-positive action. In some cases the discharges, through an uninsulated wired kite string held in the hand, have been so copious and rapid, during a haze, that it was impossible to evade a multitude of shocks. They frequently occurred in absolute torrents or vollies.

54. Now, although there may be a difference in the magnitude of the particles constituting fog, haze, and thunder-cloud,

their aqueous character is precisely the same; and it is only the elevated aerial locality and the rapidity with which the latter is formed, that gives it a superior electric tension, and enables it to discharge its electric element with violence in the shape of lightning.

55. With respect to aerial locality or altitude, I believe it is now pretty well understood that the higher regions are more densely charged with the electric element than those below. The experiments of MM. Gay Lussac and Biot, during their memorable balloon excursion in the year 1804, showed that the air, at a lower altitude, was negative to that in which the balloon was floating. The experiments which I have myself made with electric kites, at different altitudes at the same time, have invariably developed similar results: for in every case, during serene weather, the highest kite was positive to those beneath it, and the lowest negative to all those above it; but still positive with respect to the ground on which I was standing. Hence, during serene weather, there appears to be a regular increase of electric charge in the atmosphere, from the ground upwards, as some function of the altitude; and as aqueous vapour suspended in the air necessarily partakes of the same electric character, it is easy to understand that a rapid condensation of that situated at a high altitude would form cloud of a far superior electric intensity to any that could be formed nearer to the ground. Moreover, since in all cases the formation of cloud precedes discharges of lightning, and thunder-storms are frequently attended with the most violent hail showers, even during the hottest weather, there can need no further indication of a thunder-cloud's formation being the effect of a sudden depression of temperature; and, consequently, all thunder-clouds, whatever change they may suffer afterwards, are originally electro-positive. No experiment, that I am acquainted with, can illustrate this fact more correctly than that first shown by Franklin himself, with his chain and can.

56. The principles of electric action enable us to understand that the presence of a highly charged cloud will disturb the previous electric condition of the vicinal air, as well as that of other clouds of lower intensity. In the latter case, the disturbed cloud becomes prepared for the reception of a discharge from that which disturbed it: hence one cause at least why flashes of lightning more frequently occur amongst the clouds themselves than between them and the earth. This tendency to aerial lightning is also enhanced by the abundance of aqueous vapour condensing in the region of the clouds, which affords a conducting medium more easily transpierced than the interposing dry air which separates clouds from the ground, and consequently it is but seldom that lightning strikes terrestrial objects previously to the intervening aerial resistance being in a great measure removed by copious showers of rain.

57. But, notwithstanding the facilities afforded by falling rain, terrestrial objects require a predisposition to receive lightning before it can assail them. This disposition is enforced by the presence of the cloud, which disturbs the *normal* electric state of the air beneath it, by a repellency of its fluid into the ground, which will allow of its intromission in proportion as its surface is prepared for its reception. Now, allowing a plot of ground, or even an extensive tract of country, to be of an uniform conducting quality throughout, its reception for the electric fluid from the air would depend upon the character of its surface; if barren of vegetation, it could not receive the electric fluid with the same degree of facility as when luxuriously clothed with grass, corn, trees, &c., which would present myriads of conducting points and sharp edges in the best possible direction for the electric ingress, and the multitude of roots ramifying into the soil would facilitate its intromission and disposition below the surface.

58. But the electric force of a cloud capable of repelling

the fluid from the non-conducting air into the land, through the medium of its vegetable clothing, would necessarily move it still farther in this better conductor, which would necessarily become electro-negative throughout a much greater area than that presented by the lower surface of the cloud. Hence there appears but little difficulty in understanding why trees, which present multitudes of conducting points and sharp edges to the air, and stand more prominent than the ordinary level of the vegetation, should so frequently be the victims of lightning. It is, however, worthy of remark, that the tallest trees are but seldom selected; and frequently lightning strikes objects closely situated to trees without touching the latter. The nature of the soil, with regard to its conduction, as well as the objects situated on it, has much to do with the reception of lightning, for no object can receive a discharge from a cloud unless it be susceptible of electro-negation by the cloud's repellent force; hence objects situated on wet lands or on the banks of rivers are frequently those which fall victims to lightning. Heavy rains, however, render all objects, as well as the sites on which they stand, conductors, and consequently susceptible of electro-polarisation by the repellent force of a thunder-cloud.

59. Wherever land is impervious to the electric fluid, that in the air above it becomes accumulated in a stratum on its surface on the approach of a disturbing thunder-cloud, and the repellent forces of the positive stratum, and those of the cloud, cause the latter to diverge from the direction of the wind, until it arrives at a locality suitable for the introgression of the disturbed electric fluid of the air. Hence it is obvious that the removal of the electrical resistance of the air is a necessary preliminary to lightning striking objects on the earth's surface, for the force that repels the cloud is that which counterchecks the tendency of lightning discharges.

60. But if a displacement of the electric fluid of the air be a necessary preliminary to strokes of lightning, and it

be facilitated by pointed vegetable conductors, the rule applies to pointed metallic conductors also, and the tendency of lightning towards them will depend on their number and prominency, and the sharpness of their points. Hence those tall conductors which present several pointed branches to the air, are better adapted for facilitating lightning discharges than those that terminate with one point only. Hence also (57) several distinct pointed conductors, distributed over a comparatively small area of ground, or attached to different parts of an extensive building, are more likely to cause strokes of lightning in the locality than one conductor only, though furnished with the same number of points.

61. With respect to the ocean, which is a better conductor than either wet land or lakes of fresh water, its natural electric fluid would be easily displaced by the repellent force of a thunder-cloud, although not so well prepared for the reception of fluid from the air as wet land well clothed with vegetation. The presence of shipping, however, whose masts and rigging stand prominently above the surface of the water, tend much to facilitate those electric intromissions which qualify the air for the transport of lightning from a cloud to its destination; and since every portion of metal in the rigging enhances the conduction, and thus increases the tendency of lightning discharges in the direction of the ship, there appears every reason to infer that those vessels which expose pointed conductors, more lofty even than the masts themselves, are more likely to experience strokes of lightning than any other.

62. The above inference (61) appears to be somewhat countenanced by the "return ordered to be laid before the House of Lords, by precept, dated 4th May, 1848," in which it is shown that sixteen men-of-war, fitted with pointed conductors in the masts, were struck by lightning between March, 1840, and January, 1848. One of these vessels, the *Favorite*, is reported to have been struck "several times," and the *Fisgard* and *Dido* appear to have been struck twice each,

which make the number of cases twenty, by allowing three for the *Favorite*. In addition to these, there appears from another report "prepared in compliance with a precept of the House of Lords," dated June, 1849, to have been five cases (within the same period) of lightning striking men-of-war furnished with *wire-rope conductors*, which make a total of twenty-five cases within a period of eight years. The *Mindon* was struck twice, so that there are twenty-six lightning strokes in the whole.

63. In the absence of official returns to show the effects of lightning, within the same period, on those ships of the royal navy which have not been furnished with conductors, a comparison of the number of cases in which lightning has struck men-of-war, *with* and *without* conductors, may be approximated by taking the mean of the latter class of cases for eight years, out of all those that have occurred within the twenty-four years previous to 1840, which, according to a statement in the *Nautical Magazine* for May, 1844, amount to fifty-two. This gives seventeen cases for each period of eight years, which is about two-thirds only of the number of cases that have occurred to those ships to which pointed conductors are attached, a result by no means favourable to that form of conductors being introduced into the navy.

64. But if pointed conductors tend to favour the approach of lightning towards those objects to which they are attached, *oblique* discharges may find other channels of conduction, amongst those objects over which they pass, the resistance of which, at certain points of the transits, would be more easily vanquished than that offered in a direct line to the conductor itself. For, although a tall pointed conductor might be the principal object in the locality for qualifying the air so as to allow of a discharge taking place, the electrical disturbance caused by the lightning subsequent to its departure from the cloud, and the consequent electro-negations that it would enforce amongst objects within the range

of its influence, might cause it to deviate considerably from the original direction in which it started, and to take a newly-created route to its destination. Hence the probability, at least, that tall pointed conductors are frequently instrumental in bringing down strokes of lightning on vicinal objects as well as on distant parts of those to which they are immediately attached.

65. By taking this view (64) of the operations of lightning during its passage through the air, nothing seems more easy to understand than the cause of so much damage being caused by it close to the site of tall pointed conductors. Nor could anything more decisively discountenance the long-fostered idea that the *path* of lightning and the objects which it strikes are definitely marked out previously to its leaving the cloud.

66. If, therefore, it be allowable to lay to the charge of tall pointed conductors those cases of damage by lightning which have occurred to vicinal objects (19—25, &c.), their reputation, as protectors to shipping, will appear less favourable than from the estimate already made (63). For the cases of damage which would thus be attributed to them would, within a similar period, be more than double the number of those that have occurred in their entire absence, or where they could have no influence whatever.

67. Another important argument arises from the *effects* that lightning has been known to produce on pointed conductors. The most notable of these is the destruction of the points by fusion, in those cases where they have received the discharge. Instances of this kind have been noticed from the earliest period of lightning conductors to the present time. Several have occurred to conductors attached to buildings; and marine conductors have frequently met a similar fate. In the report already alluded to (62), it is stated that in the case of H. M. ship *Constance*, "the *main spindle was*

fused," and that "the conductors on the fore and main masts were fused in several places."

68. In the case of the *Fisgard* (41), the same report states that "the end of the *vane spindle on the main-mast was fused*, and the conductors aloft exhibited small spots of fusion. The copper plates were started from the mast for a few inches, about 13 feet above the deck." The report of Captain Duntze, who commanded the *Fisgard* at the time, is somewhat more minute, and states that the mast itself was "slightly singed" and splintered about the place where the conductor was started from it. (See 41, for further particulars.)

69. With respect to the effects below deck, Capt. Duntze states that, "The electrical current having passed down the main-mast, took the direction of the branches to the bolts through the side,—one leading through the boatswain's cabin, and the other through the midshipmen's berth. The branch conductors in this ship, instead of leading directly to the copper sheathing near the water's surface, as originally proposed, had been led out above it, to two bands of copper passing down externally over the ship's side; these bands were also started at the ends in contact with the termination of the through bolts; the copper sheathing covering the other extremity of the band was bulged outwards. It appears further, by other reports, that at the point of contact with the branches and iron knees within the ship, the metal was blackened, as if a slight expansive action had occurred at these points; this is said to have been more apparent in the boatswain's cabin, and may have arisen from an imperfect contact with the iron knee and through bolts."*

70. The statements in Captain Duntze's report are of an exceedingly interesting character, because of their furnishing the particulars of the several events which occurred during

* Captain Duntze's Official Report. Harris's Remarkable Examples.

the transit of the lightning from the mast-head to the sea, some of which, to an electrician of the present day, would appear to be sufficiently alarming to discountenance *in toto* this particular plan of conductors, notwithstanding that they were originally projected by Mr. Henley, one of the ablest practical electricians of the last century,* who, no doubt, was led into the error from a want of more experience than could possibly be afforded at the time he proposed them. In a paper, by this excellent electrician, which appears in the *Transactions of the Royal Society*, the author states that if instead of chain conductors "plates of copper $\frac{3}{10}$in. thick and 2in. broad, with the edges neatly rounded off, were inserted in a groove and continued down the main-topgallant-mast, the main-topmast, and part of the main-mast, into the well-hole; a communication from the mast to the underside of one of the decks might be made with a plate of metal flattened at each end, and from that rod the conductor might be continued by plates of lead or copper on the underside of the deck and down both outer sides of the ship as low as the keel, if it be thought necessary; and this method, I should apprehend, would be preferable to the chains, now in use. Particular care should be taken to have all the plates which form the conductor as nearly as possible in contact with each other, and to fix a sharp pointed slender rod of copper at its summit. And, for the purpose of connecting the plates inserted in the main-topgallant-mast, the main-topmast, and main-mast, if a hoop of copper were fixed in a groove of its own thickness at the top of the main-mast, and another such hoop at the upper end of the main-topmast, perhaps they might answer this end very conveniently."

71. It seems pretty clear, from Captain Duntze's official report alone, that explosions took place below deck as well

* See Mr. Henley's paper on this subject, in the Philosophical Transactions of the Royal Society for 1774.

as on the mast above, for nothing less than a high degree of temperature, occasioned by an explosion, could blacken the metal at the junction of the iron knees with the branch conductors. And this view is still further supported by a letter (from an officer belonging to the ship) that appeared in the *London Illustrated News* for March, 1847, in which it is stated, that "on reaching the lower deck the discharge took the branches under the beams leading to the bands on the ship's sides; *these were started* at the ends in contact with the copper bolts leading to the sea, and the copper sheet covering the joint was *bulged upwards* by the expansive force of the shock."

72. Nor is this a solitary case of explosions occurring below deck, by lightning traversing the branch conductors. In the case of H. M. ship *Constance*, already alluded to (67), it is officially reported, that besides the injury the conductor received on the mast, the lightning *slightly* tore "away part of the casing which covered the branch conductor in the boatswain's cabin, close to the side where it escaped."* H. M. ship *Fox*, 44 guns, was struck by lightning April 5th, 1847. "Lightning passed down the chain conductor, *passing visibly through midshipmen's berth* and *Commodore Steward's berth*, and leaving a *burnt mark*."† H. M. ship *Conway*, 26 guns, was struck by lightning March 9th, 1846. The electric fluid "traversed the branch or beam in the gunner's cabin, who experienced no ill-effect; he saw it pass from the ship in a blaze of light. Mr. Lethbridge, officer of the watch, then on deck, describes the effect as if *one or two of the main-deck guns* had been *fired up the hatchway*."‡

73. The effects thus described to have taken place *below deck* (67—70), are of a similar character to those observed on the masts above deck; and from the same cause, viz., a want

* Official Return, prepared in compliance with a precept of the House of Lords. Dated 11th June, 1849.

† Ibid. ‡ Ibid.

of continuity in the metallic channel, which will always allow of explosion when lightning discharges traverse conductors made up of metallic plates. Hence the separation of "the plates of copper forming the conductor" of the main-mast ot the *Fisgard* (41), and of those also in the branch conductors as well as the starting of the copper bands from the exterior ends of the through bolts, and the bulging *outwards* of the copper sheathing (69), were results that might easily have been foreseen as some of the natural consequences of such explosions.

74. But if an ordinary discharge of lightning is capable of producing such fearful explosions within the ship, and ruptures in the conductors and copper sheathing, we have only to allow of a second discharge of lightning to strike the ship before the injured conductors were repaired, to form a picture in the mind of the serious consequences likely to ensue. Ruptures partially accomplished by the first discharge would be completed by the second, and former breaches would be widened; and many dangerous explosions, both above and below deck, would probably take place.

75. Should this plan of conductors be introduced to the merchant navy, there would be no predicting the evils that might arise, even from the first flash of lightning that traversed them, especially when the cargoes consisted of cotton-wool, hemp, flax, &c., close jambed against the branch conductors at those places most likely to experience explosions. And as it would be next to impossible to accomplish a minute inspection of these branch conductors when most necessary, the chances of avoiding danger from subsequent strokes of lightning would be much less than in men-of-war, where every facility for discovering breaches in the conductors, and of repairing them by competent hands, is always afforded.

76. It appears, therefore, from a due consideration of all the facts that experience has hitherto furnished, that there

are two fundamental errors, fraught with great danger, in this plan of conductors, the most palpable of which is that of leading the lightning into the hull of the ship, and then having to incur a heavy expense in endeavouring to get it out again. The other is that of having the conductors compounded of many constituent pieces, although copper, of any required dimensions and form, is always procurable.

77. Although I am far from thinking that conductors let into the masts are the best situated for protecting ships from lightning, still, should that plan continue to be persevered in, they ought not, under any consideration whatever, to proceed downwards on the masts farther than the upper deck. With conductors thus applied, there could be no apprehensions entertained of explosions taking place below deck, nor of chronometers being injured by the magnetizing influence of lightning, which is also a consideration of importance. Moreover, as each individual mast would have an uninterrupted conducting flat metallic rod throughout, there could be no explosions in the circuit excepting such as might occur at the juncture of the rods, at the heads of the lower and top-masts, or at those other places where lightning might strike the mast below the highest point. The examination of those parts, and their repairs, if necessary, would require neither much time nor skill to accomplish.

78. I am not aware of the manner in which the wire-rope conductors have been attached to the several men-of-war that have been fitted up with them; but it appears, by the official report, that although they have answered the purpose exceedingly well in some cases, they have entirely failed in others. Commander Darley, of H. M. ship *Electra*, reports favourably of these conductors, having observed their efficiency in carrying off several discharges of lightning, whilst on the West India station, in the year 1842. But, on board the *Hazard*, where similar conductors were employed, they have failed to protect the ship on two occasions, viz., May 1st and

June 12th, 1846. It is worthy of remark, however, that although the mast and some parts of the rigging suffered much from the lightning, it was safely conducted overboard on both occasions. The damage suffered by the *Hazard*, June 12th, 1846, is very likely to have arisen from these very circumstances. "Lightning struck the ship, splitting and carrying away main-topgallant and royal-mast, the whole of the main-topmast from the hounds to the lower cap, damaging the top, and sprung the after cross-tree, split and carried away starboard tressel-tree; a splinter from main-topmast passing through the starboard side of the quarter-deck, the electric fluid escaping from the conductor by the main rigging overboard." In this case, nothing is said about injury to the wire-rope conductor, which, no doubt, was on the opposite side of the ship to that on which the lightning entered the rigging; and it is very likely that the lightning was very much attenuated amongst the masts and other parts which it destroyed or damaged, before any part of it arrived at the conductor. Indeed, the whole force of the discharge could never arrive at the conductor, because much of it would be carried off by the main-mast to the deck, and thence to the sea.

79. If the *Hazard* were fitted with conductors in the manner that Commander Darley describes those of the *Electra*, viz., one to each mast, they would be still more liable to entail danger on the rigging than conductors fitted into the masts; because an approaching discharge from the other side of the ship, if it did not attack the conductor at the mast head, would have to pass through some part of the rigging before it could arrive at it. This must always be the case where the rigging is protected on one side only.

80. But there are other serious objections to the wire-rope conductors, (unless very differently applied than on board the *Electra*,) which Commander Darley says "are composed of copper wire-rope, one to each mast; they are passed through

the mast head, trucks, and caps, and lead down the back-stays through the channels, and are attached to a copper plate secured under the ship's copper." *

81. The principal defect in the present fashion of the wire-rope is the diminutive size of its strands, which are easily fused individually, on that side especially which receives the lightning stroke; and if the discharge be very formidable, the whole of the strands might be blown to atoms, or torn asunder at the point which received it. Such seems to have been the case with the conductor of the *Hazard*, for on one of the occasions when the ship was struck by lightning, it "carried away the conductor at main-topgallant-head."† The wire-rope at present in use, besides the defects already pointed out, is twisted a great deal too much, for it requires a greater length for the strands than there is need for, and the more compact they are laid together (for they can never form one solid body) the more subject they are to be blown asunder by lightning. The only object for having wire-rope is to maintain pliability, which is attainable to any required extent for marine conductors, by the employment of thicker strands, which would be easier laid or twisted together.

82. But as oblique discharges of lightning frequently strike conductors below the mast-head, wire-rope will always be liable to damage under these circumstances, because some of its strands are almost sure to suffer by every formidable discharge. The only danger, however, would be at the point of attack, which is more likely to be sustained by moderately thick wires than by thin ones; but, in all cases, both sides of the rigging ought to be protected (76), and the conductor on each mast ought to be metallically connected at the cross-trees and at the tops, as well as at the topgallant-mast-heads. By these means, oblique flashes of lightning, from whatever

* Return, &c., prepared in compliance with a precept of the House of Lords Dated 18th June, 1849.

† Ibid.

quarter they came, would meet with conductors without having to enter the rigging, and the conductors would act in concert, each branch carrying off a portion of the discharge; and if to these, other conductors were to connect the several pairs of starboard and larboard conductors, either along the stays or otherwise, the whole system would act in concert, so that however formidable the attack might be on any one point, the distribution would be general throughout the system, and the discharge into the sea from each individual branch would be perfectly harmless. A want of metallic connection above the lower masts of the several conductors attached to the masts individually, is a serious error, whatever may be the character of those conductors or the plan of fitting them up. For whilst they are insulated from each other, they act separately and independently, and derive no conducting assistance whatever from each other, each having to sustain the whole force of the lightning that strikes it, from the point of attack to the sea.

84. There is also an objection to the method of applying the rope conductor to the mast-head, as described for the *Electra* (77), because of the interruption that must necessarily occur betwen the rope and the pointed vane spindle. For, whenever lightning strikes the spindle, there will be an explosion between it and the rope which may be sufficiently formidable to destroy the rope at that place. It is even probable that this was the case when H. M. ship *Hazard* was struck, May 1st, 1846 (79), for the *Report* states *that* the lightning "carried away conductor at main-topgallant-head."

85. From the fact that the points of conductors are frequently fused by lightning, and the great probability, at least, that pointed conductors predispose their localities for the reception of lightning, and thus facilitate its discharges in their own direction, there appears good reason for abandoning the employment of points altogether. For if they had

answered even any of the purposes for which they were first introduced, the first flash of lightning that any of them received would destroy it. Such an accident on board of ship might be but of little consequence, because the vane-spindle being accessible could easily be pointed anew; but a similar accident to the conductor of a tall factory chimney, would entail a heavy expense in scaffolding, &c., before the top of the conductor could be arrived at; so that the supposed virtues of points is an expensive treasure, in continual jeopardy. Moreover, as there has never yet been an instance known of pointed conductors abating the violence of lightning discharges, there is not an attribute belonging to them, nor a recorded circumstance in their favour, that can justify a continuance of their employment in preference to conductors of other forms at their superior extremities; whilst the grave charge of their being instrumental in facilitating strokes of lightning, almost imperatively demands their immediate and total discontinuance.

86. Notwithstanding the attempts that have been made to propagate the idea that *balls*, surmounting conductors, are virtually *points*, when compared to the immense surface of a thunder-cloud, and (by inference) act in a similar manner, the result of Franklin's experiment with the point and head of a pin alternately presented to the prime conductor of his machine would, independently of any other fact, be sufficient to show the fallacy of the hypothesis. But the inaptitude of polished balls in drawing off the electric fluid from vicinal bodies, or intromitting it from the air, is a fact too well established at the present day to admit of either equivocation or doubt. Hence such terminations to conductors are incapable of qualifying the air beneath a thunder-cloud for the transmission of lightning; and consequently have no power in predisposing the locality for its reception. These negative qualifications, in connection with the certainty of their with-

standing any flash of lightning that may assail them, are strong recommendations for the employment of globular terminations to conductors. They offer precisely the same amount of protection as pointed conductors, should lightning come in their way, whether the discharges be vertical or oblique; and as they can never be accessory to its approach, they possess important advantages over those of the usual form.

87. I know of no better *general* guide for the application of lightning-rods, than that embraced in the following motto: *Offer no facilities for the approach of lightning; but prepare for its reception in case it comes.*

88. With respect to the practical application of conductors to buildings, no individual plan will answer for every fashion of edifice. The most simple *systems* of conductors are those that would be applicable to cottages, powder magazines, steeples, tall factory chimneys, &c., for which some general rules might be given; but buildings with complicated irregular roofs, and decorated gables, chimneys, &c., would require a peculiar plan of conductor for each fashion of house. But no building, however plain in its structure, can be rendered secure against lightning by one individual rod only.

89. I have devised the following system of conductors for powder magazines. It consists of three vertical arches of cylindrical copper rod, which are united by a horizontal rod of the same metal, and furnished with three balls.

90. Steeples, of the form of square towers, ought to have a vertical copper rod at each angle, united by a copper band at the top, or just above the uppermost weather mouldings. The vertical rods should reach to above the highest pinnacles, and be surmounted with copper balls; or the whole might unite in one ball directly over the axis of the steeple.

91. Spires may be protected by three copper rods united in a ball at the apex of the cone, and reaching downwards,

at equal distances from each other, to the base, where they should be again united by a copper band. From any part of this band a single rod, reaching to the ground, would complete the conducting system.

92. Tall factory chimneys would require three rods from top to bottom. They should be united in a ball directly over the axis of the chimney, in the fashion of a bird-cage, and again by means of a copper band about half-way between the coping and the ground.

93. In all cases of applying conductors to buildings, the rods should be kept clear of the masonry, at least 3in. or 4in., which may easily be accomplished by allowing them to pass through oaken holdfasts built into the wall. There can be no reason shown for the employment of deep pits or wells, for the reception of the inferior extremities of conductors, where brooks, drains, or gutters are near at hand; for lightning can be safely dismissed by continuing the rods horizontally, just beneath the surface of the ground, to a few yards from the building, and terminating them by a number of sharp metallic points in any of those channels, which are almost sure to be well supplied with water before any lightning descends from the clouds.

94. By employing a *system* of conductors, instead of a single rod, protection is insured against oblique discharges, from whatever quarter they may proceed; and the union of the several branches allows of a distribution of the force throughout the whole system. The spire turret of St. Chad's Church, Cheetham Hill Road, Manchester, which was much injured by lightning on the 23rd of May, 1850, has been furnished with a system of conductors, similar to that described (91), under my own superintendence. The *main* rod, which reaches from the base of the spire to the ground, is continued about 6 yards under ground, at right angles from the side of the tower, and is bent down into a water-drain.

95. A circumstance of an exceedingly interesting character, and replete with valuable instruction, occurred when this church was struck by lightning. When the lightning left the injured turret, it sprang from one iron cramp to another, demolishing the elegant ornamental masonry on the parapet on its way to a leaden gutter, which conducted it to a cast iron water-pipe situated in one of the angles of the tower, and reaching from the top down to the roof of the church, round which are leaden gutters for conveying the rain-water to other cast iron pipes which reach the ground. Whilst on the top of the tower, examining the nature and extent of the damage, I had some difficulty in discovering the route of the lightning, until the sexton who was with me, informed me of the iron pipe inside of the tower. On returning to the bell-loft, I examined the water-pipe very minutely, under the expectation of finding it damaged by the lightning, but I could not discover the slightest breach; the sexton told me that a great quantity of water had fallen on the floor below (the floor of the ringing loft), but it was not known where it came from. We descended to the place, and I soon discovered that one of the pipes was broken and a large piece of it was missing. On looking on the floor at the opposite angle, we found several fragments of the pipe which had been blown from the injured part. At my request, the remaining portions of the cast iron pipe were examined, from the top of the steeple to the ground, and three or four of the *lengths*, at their junction with one another, were similarly damaged.

The suspicion of some damage having been done to this water channel, arose from my knowing that the several *lengths* of which it is composed are insulated from one another by means of the cement (generally stiff white-lead paint) employed at the joints, for rendering those parts water-tight. These interruptions in the circuit will always be liable to lightning explosions; and, of course, endanger the pipe at

every joint. Nor does a full flow of water through them appear to render sufficient assistance to prevent their being damaged, for, in this case, the rain was falling in torrents during the whole time that lightning was in the neighbourhood of the church, and the pipe must have been conveying a copious stream at the time it was damaged. It is probable, indeed, that the presence of a stream of water would increase the danger; because of a sudden production of steam by explosions at the joinings of the *lengths*.

It is worthy of remark, also, that this iron pipe is comparatively new, for the church has not been finished more than about three years. I have some of the fragments in my possession at this time, and I find they are painted black outside, and are perfectly sound on the inner side. I observe, also, that the *lengths* have been painted before being fitted into each other; so that they are insulated from one another by a coat or two of paint in addition to cement.

This circumstance is, I believe, the first of the kind that has been noticed; but it is too important to allow of its omission in this place. The rule which has, for a whole century, guided lightning-rod manufacturers, to avail themselves of every portion of metal that may happen to be conveniently attached to the building can no longer be depended on for protection; for, unless every portion of metal thus employed, were securely and perfectly united, explosions might take place on the roof, or against the walls, which would be productive of serious damage.

The spire, and other portions of St. Chad's Church (95), as has been the case with many others, suffered by explosions from metal to metal; and if a second discharge of lightning had passed along the broken water-pipe before the damage occasioned by the first had been repaired, the explosions at these interruptions in the circuit would probably have displaced large portions of masonry from the tower, as

well as from the side wall of the church, against which the pipe is placed.

I should be exceedingly glad to hear that the description of this event, and the remarks I have made upon it, were the means of providing better security to her Majesty's palace at Osborne, than that afforded by cast iron water-pipes, eight or nine of which are now jeopardising the building, by being employed as portions of as many distinct lightning conductors.

II.—*On some Peculiarities of the Thunderstorm which occurred in this neighbourhood on Tuesday, the* 16*th of July last.*

By the late Mr. William Sturgeon, Prestwich.—(Communicated by Mr. Joule.)

[*Read March 4th,* 1856.]

The principal characteristics, of this storm, that I have to notice in this communication, are, unusual displays of lightning, and the effects of electrical disturbances caused by it at a distance from the primitive discharge.

This storm, which commenced in the afternoon, was experienced on every side of Manchester at nearly the same time. On the road near to Bury, the lightning killed a young man and the horse he was riding on. It was exceedingly violent over Bolton and Blackburn; also at Liverpool and Northwich. As the principal part of the storm never reached this place, I had an opportunity of exploring the atmosphere, by means of an electric kite, without apprehension of danger, although many thunder-clouds passed slowly over during the time. I got the kite afloat, with 300 yards of string out, a little before six in the evening, and kept it up till nearly eight. During this time the storm was raging over Bolton. The lightning was very frequent and brilliant, notwithstanding the bright daylight it had to contrast with.

The discharges from the kite-string were occasionally both frequent and powerful, and at other times, but seldom and feeble; which is frequently the case in the vicinity of thunderstorms. But in all cases, whether the sparks were large or

small, they were invariably positive; showing that the air, in this instance, retained its *normal* electric character (its electric state during severe weather), though much disturbed by neighbouring clouds and distant lightning. With respect to the disturbance from the latter cause, it was very marked on the present occasion, and its effects, in some of the instances I have to mention, although not often observed, are such as are always to be dreaded from vicinal flashes of lightning.

During my experiments at the kite-string, I had the company of the Rev. W. W. Johnson, who assisted me in carrying them on. After ascertaining the electric state of the air and its vacillating intensity, my attention was drawn to the distant lightning, which was now very fine; and its disturbing influence was soon discovered to extend to the place of observation. The wire connected with the ground was placed at a certain distance from the conductor of the string, and the frequency of the sparks observed. At times no sparks were transmitted, at other times an abundance of them were discharged through the interval within a few moments. On comparing these fluctuations with the intervals between the lightning flashes, we each took a separate part in the observations. I looked out for the lightning, and announced each flash by the word "*when;*" whilst Mr. Johnson kept his eye on the apparatus. Whilst I was silent, scarcely any sparks appeared; but a shower of them were discharged from the kite-string whenever I announced a flash of lightning: thus showing by the concurrence of the events, that there existed a rigid connection between the *primitive* and *secondary* discharges.

Phenomena of this class are by no means of recent discovery, though not often shown in this manner; and have generally been considered as *primitive* rather than secondary events. The easiest way of ascertaining an electric disturbance, by the influence of a distant flash of lightning, is by

holding the base of an electroscope in the hand, and elevating it in the air above the head during a thunderstorm; every flash of lightning will produce a sudden divergency of the gold leaves. When the lightning is near, the disturbance is too powerful for a delicate electroscope to withstand.

On the day following that of the storm, I had information of a cotton mill being set on fire by the lightning, at Bolton. I immediately proceeded to the place, for the purpose of examining the effects of the lightning on the mill, which I found to belong to Messrs. Bolling and Co., and situated in Bradshaw-gate. By the kindness of the foreman I soon gained admittance, and had every attention paid to my inquiries. I was shown that part of the machinery which had been on fire, but no indication could be found of lightning having entered the building.

The damage was very trifling, being limited to a carding machine and a few boards of the floor above, which were charred by the fire. This limitation of the damage, however, was owing to an incidental circumstance which fortunately occurred, otherwise it is probable that the whole of the premises would have been destroyed.

The rule observed at the mills is to turn off the steam-power at half-past five daily, and the people leave the place at six. On this occasion, however, the rain was so unusually heavy, and the lightning and thunder so terrific at six o'clock, that no one would venture out. Shortly after six, a terrific flash of lightning took place, attended by a crash of thunder; and immediately an alarm was given that the mill was on fire. Water buckets were immediately served out, and all who could assist were employed in extinguishing the fire, which was accomplished within a few minutes, and before it had time to spread far from where it began.

The very natural conclusion was that the mill had been struck by the lightning, though no breach could be discovered in the roof or walls, nor was a pane of glass disturbed. I

made very particular inquiry about every circumstance that occurred that I considered of importance; all hands thought the mill was struck, but nobody could find the place. Some thought the whole building was on fire for a moment, and those in the rooms thought the lightning struck every part of the machinery. The fact is, the lightning never entered the building at all; and it is obvious, from all the circumstances of the case, that the fire originated from a disturbance of the electric element naturally belonging to the machinery; probably by the influence of the individual flash of lightning already alluded to. Such a disturbance amongst the metallic parts of the machinery would probably give rise to multitudes of sparks, each of which would be sufficient to ignite cotton, wool, or other inflammable material, that happened to be in the way; and, as in this case, the fire originated amongst some loose cotton attached to the carding machine, it is easy to understand how its ignition occurred. I was particularly anxious to ascertain the initial point of ignition, and from the information I received respecting the burning cotton, and the communication of the fire to the floor above, the lower surface of which being that only which had suffered, there appeared no difficulty in understanding where the fire originated.

In addition to the indications already noticed, I had direct evidence of the existence of sparks by their effects on a roller closely situated to the site of the fire, but not in immediate connection with it. This roller is of wood, about 4in. long, and 1¼in. diameter; its hollow axis is filled with lead, and its surface covered with flannel. (I am not aware of the name of this roller, but I send it with the paper for inspection.) The several singed spots on the flannel covering are so many evidences of local fires on the roller's surface, which could not be produced by any ordinary mode of ignition. Nor can it be supposed for a moment that the fire originated from friction, because the machinery had been at rest for more than half an hour; besides which the parts ignited on this roller are not

such as would be likely to be produced by friction, if even it had been in motion. Moreover, the motion of this part of the machine being slow, and its surface not very inflammable, its ignition from friction seems quite out of the question.

From this mill I was directed to another, situated in Halliwell-street, and belonging to Mr. Thomas Cross, which I was told had been set on fire by the lightning. As the people were leaving work when I arrived, my only guide in the mill was the porter, who, after a barrier of prejudice and prudent caution had been removed from between us, became exceedingly polite and communicative. The occurrence here seemed to be simultaneous with that at the other mill. I was first shown a broken window at the landing to the uppermost floor of that part of the mill, and also a rupture of the plastering in the corner close to the ceiling. Only a small portion of plaster was displaced, but the stones* which it had covered were partially shattered. This damage was directly over the burner of a thin leaden gas-pipe, and about two feet from it. As no other damage could be discovered, it appeared obvious that an explosion had taken place between the wall and the gas-pipe; and as no mark of fusion could be discovered on the metal, the quantity of electric fluid transmitted was not great. The explosion, however, would produce a momentary expansion of the air, which would be sufficiently violent to break the window. My guide, however, insisted that the lightning had come through the window, and had melted the glass into smoke, for not a single fragment of it could be found. I told him to look outside on the window-sill, and perhaps he might find some, which he soon discovered to be true; but what astonished him most was, how I, who had never been in the mill before, should know where to find the broken glass. After allowing him to exercise his imagination for a minute or two, I relieved him by pointing out the cause.

* It is a stone building.

He became quite delighted with this piece of information, and immediately took me to another part of the mill where a quantity of cotton had been set on fire on the drums of two carding machines. This accident occurred close to a window which had suffered no damage whatever, nor was there to be found any trace of the *direct* action of lightning. The two drums on which the cotton was ignited are distant about forty yards from the gas-pipe already mentioned, and as the fluid that struck the burner would follow the pipe to the ground, which was in a direct line downwards to the metre on the ground floor, and thus to the underground main, it would be impossible to imagine that any *direct* action from that flash of lightning on those distant carding machines could take place; and, as not the slightest damage was done to any part of the mill beyond that already mentioned, at the distance of forty yards, there appears no other mode of accounting for the fire than that arising from a consideration of the effects of electrical disturbances by vicinal flashes of lightning.

These effects of electrical disturbance are in direct correspondence with many others, of the same class, that have been observed, but hardly ever clearly accounted for. Such are deaths from lightning where no external marks of violence appear on the subjects; the singeing of garments without injury to the wearer, beyond that of a momentary shock, &c. In many cases, however, the expansion of the air and the sudden conversion of fluids into steam and gases are productive of much damage, and even of death.

Every flash of lightning necessarily disturbs all the electric fluid around it, and thus produces an *electrical wave*, which, in some instances, reaches to great distances, causing a sudden increase of pressure, and a consequent intromission of fluid to the ground through the conducting vegetable points and sharp edges with which it is covered. Now, we have only to imagine a tree, which exposes a large area of foliage from its branches, to be situated near to the track of a heavy flash

of lightning, to form an idea why the trunk should be decorticated and split to shivers, whilst no mark of violence appears on the leaves and branches; for the suddenly disturbed electric element in the vicinity of the tree would find an easy ingress through the medium of the foliage, which, in consequence of each leaf and point introducing but a small portion of the whole that entered the tree, would not suffer by the transmission from the air; but as the whole of these portions would arrive at the trunk at the same moment, they would form a formidable mass of the electric element, sufficient to convert the sap into steam, with an expanding power that would accomplish the tree's destruction in a moment.

But to return to the Prestwich observations. After leaving the ground where the kite experiments were made, the lightning appeared so unusually fine that I was induced to observe it for a long time; and as the flashes, or rather streams, of electric fluid were each of longer duration than any I had previously seen, I stept into the house for a pendulum to ascertain the time which each discharge occupied, and found that, in some cases, the electric fluid was visible during a second and a half. Most of the discharges seemed to be at great altitudes amongst the clouds, and had a fiery red appearance; and as several appeared to shoot upwards, they, according to the rules of perspective, proceeded *towards* the place where I was standing. Many discharges, however, took a horizontal direction at right angles to the former, and others were seen to shoot obliquely in various directions. The streams of electric fluid were of unusually large diameter, and seemed like streams of liquid fire; and in some instances they looked like sky-rockets, with a burst of multitudes of stars. Upon the whole, this was the most singular display of lightning I ever beheld.

III.—*On the Comparative Value of various kinds of Stone, as Exhibited by their Powers of Resisting Compression.*

By W. FAIRBAIRN, F.R.S., &c.

[*Read April 1st*, 1856.]

OUR knowledge of the properties of stone, viewed as a building material, is very imperfect, and our architects and stonemasons have yet much to learn concerning the difference between one kind of stone and another, both as regards their chemical constitution, their durability, and their powers of resisting compression. On this subject we have the experiments of Gauthey, Rondelet and Rennie, which to some extent supply the deficiency and furnish data for the resistance to a crushing force of a considerable variety of stone. These are, however, to some extent inapplicable to the purposes for which such data are required, and not finding them in exact accordance with the results of some experiments recently made, I have endeavoured to inquire into the causes of the discrepancy, and to account for the difference.

Stone is found in various forms and conditions, embedded in and stratified under the earth's surface. That portion of it which is used for building purposes, is a dense coherent brittle substance, sometimes of a granulated, at others, of a laminated structure. These qualities varying according to its chemical constitution and the mode in which it has been deposited. Sometimes the laminated and granular rocks alternate with each other; at others, a rock of a mixed form

prevails, partaking of the characteristics of both structures. Independent of these properties is its power of resistance to compression, which depends chiefly upon its chemical combinations and the pressure to which it has been subjected whilst under the earth's surface from the weight of superincumbent materials. The granite also, and other igneous rocks, owe their hardness to their having crystallized more or less rapidly from a fused mass.

In attempting to ascertain the ultimate powers of resistance of rocks which have been deposited by the action of water, it is necessary to observe the direction in which the pressure is applied, whether in the line of cleavage, or at right angles to it. In nearly all of the following experiments this precaution was attended to, and it will be seen that the strength is far greater when the force is exerted perpendicularly to the laminated surface, than when it is applied in the direction of the cleavage. In building with such stone, it is also important that it should be laid in the same position as that in which it is found in the quarry, as the action of rain and frost rapidly splits off the laminæ of the stone when it is placed otherwise. The strength of the igneous, or crystalline rocks, is the same in every direction, owing to the arrangement of the particles of which they are composed.

It might have been advantageous to have ascertained, by analysis, the chemical composition of the substances experimented on; but as this varies in almost every locality, and that in accordance with the superincumbent and surrounding strata, this is of less consequence in practice than a knowledge of absolute facts in connexion with the properties of the material. Deductions from direct experiment are of no small importance to the architect and builder, as he should not only be acquainted with the strengths and other properties of the material on which he works, but also with the changes of those qualities under the varied forms of stratified, metamorphic, and igneous rocks.

On the durability of the specimens, I have made no further inquiry than in regard to their power of resistance to strain. Any addition would require a separate investigation into the chemical constituents of the different specimens, and into those changes to which stone of almost every description is subjected when exposed to the action of the atmosphere. In omitting this branch of the investigation I have not forgotten its importance, but have very properly left its development to abler hands.

Before giving the results of the inquiry, I may observe that a portion of the experiments were undertaken at the request of Mr. E. W. Shaw, the surveyor of the borough of Bradford, in Yorkshire, in order to ascertain the best and strongest qualities of stone for paving the streets of that town. The following tables give the result of the experiments on fifteen specimens of Yorkshire sandstone, and on some specimens from Wales and other places, as follow.

Experiments to determine the force necessary to fracture, and subsequently to crush, 2in. cubes of sandstone from the Shipley quarries, Bradford. The pressure applied in the direction of the cleavage.

No. of Expt	Weights laid on in lbs.	Remarks.	No. of Expt	Weights laid on in lbs.	Remarks.	No. of Expt	Weights laid on in lbs.	Remarks.
Specimen No. 1. Shipley.			Specimen No. 2. Heaton.			Specimen No. 3. Heaton Park.		
12	31732		11	31732		8	26356	
13	33524	fractured	12	33524	fractured	9	28148	
...	......	......	...	......	.....	10	29940	fractured
16	38900	crushed	16	40692	crushed	11	31732	crushed
Specimen No. 4.			Specimen No. 9. Old Whatley.			Specimen No. 10. Manningham-lane.		
This specimen was defective and crushed as the first weight, 28148 lbs., was laid on.			11	31732		8	26356	
			12	33524		9	28148	fractured
			13	35316	fractured	...	......	......
				& crushed	suddenly.	14	37108	crushed

The results of the experiments 1, 2, 3, 9, 10, fractured and crushed in the line of cleavage, are given in the following table.

No. of Specimen	Locality.	Size.	Weight at which it fractured.	Weight at which it crushed.
1	Shipley, Bradford.	2in. cube	33524	38900
2	Heaton.............	„	33524	40692
3	Heaton Park.......	„	29940	31732
9	Old Whatley.......	„	35316	35316
10	Manningham-lane..	„	28148	37108
	Mean.		32090	36749

Experiments to determine the force required to fracture, and subsequently to crush, 2in. cubes of sandstone from the Shipley and other quarries, near Bradford. Pressure being applied at right angles to the cleavage.

No. of Expt	Weights laid on in lbs.	Remarks.	No. of Expt	Weights laid on in lbs.	Remarks.
Specimen No. 5. Idle Quarry.			Specimen No. 6. Jegrum's-lane.		
15	38900		18	44276	
16	40692		19	45172	fractured.
17	42484	fractured.	...	......	....
18	43380	crushed.	22	47860	crushed.
Specimen No. 7. Spinkwell.			Specimen No. 8. Coppy Quarry.		
10	29940		14	37108	first fracture..
11	31732	fractured.	...	..... .	....
...	......	....	16	39796	second fracture.
14	37108	crushed.	...	......	....
			18	41588	crushed.
Specimen No. 11 failed.					

Results of experiments on specimens 5, 6, 7, 8, fractured and crushed at right angles to the cleavage.

No. of Specimen	Locality.	Size.	Weight at which it fractured.	Weight with which it crushed.
5	Idle Quarry, Bradford...............	2in. cube.	42484	43380
6	Jegrum's-lane	"	45172	47860
7	Spinkwell...........	"	31732	37108
8	Coppy Quarry......	"	37108	41588
	Mean.		39124	42484

By the foregoing experiment it will be observed that the resisting powers of stone to compression, are greatest when the pressure is applied perpendicularly upon the bed or laminated surface, and that in the ratio of 100 : 82 in the force required to fracture, and 100 : 86 in the force required to crush this description of stone. Hence, as already observed, the powers of resistance of every description of laminated stone, are most effective when the beds are placed horizontally or perpendicularly to the direction of the pressure, and this position is the more important when the stone is exposed to the atmosphere, as it partially prevents the absorption of moisture, which in winter tends to destroy the material by the contraction of the stone and the expansion of the water at low temperatures.

Experiments to determine the force required to fracture and crush* 1*in.*, 1½*in.*, *and* 2*in.* *cubes of stones from Scotland, Wales, and other places.

No. of Expt	Weight laid on in lbs.	Remarks.	No. of Expt	Weight laid on in lbs.	Remarks.
Specimen No. 12. Grauwacke. Penmaenmawr, Wales. 2in. cube.			Specimen No. 14. Granite. Mount Sorrel. 2in. cube.		
16	40692	slight fracture.	19	46068	
...	....	....	20	47860	
29	63988	second fracture.	21	49652	
30	65780		22	51444	fractured, and after a slight rest crushed.
31	67572	crushed.			

Specimen No. 15. Grauwacke. Ingleton. 2in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
13	35316	first fracture.
...	....	....
20	47860	second fracture.
...	....	....
25	53236	not crushed.

Specimen No. 16. Granite. Aberdeen. 2in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
8	26356	
9	27546	fractured.
10	28148	
11	28340	not crushed.

Specimen No. 17. Syenite. Mount Sorrel. 2in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
17	42484	
18	44276	
19	46068	
20	47284	crushed.

Specimen No. 18. Granite. Bonaw. 1½in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
2	15604	
3	17396	fractur'd in 2 nearly eq. pts.
...	....	
7	24564	crushed.

Specimen No. 19. Furnace Granite. Inverary. 1½in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
4	19188	
5	20980	
6	22772	
7	24564	crushed.

Specimen No. 20. Granite. A. 1½in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
4	19188	
5	20980	
6	22772	fractured.
7	24564	crushed.

Specimen No. 21. Limestone. B. 1½in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
1	13812	
2	15604	
3	17396	fractured.
4	19188	crushed.

Specimen No. 22. Limestone. C. 1½in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
2	15604	
3	17396	
4	18292	fractured.
5	19188	crushed.

Specimen No. 23. Magnesian Limestone. Anston. 1in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
1	1258	
2	2154	fractured.
...	......	....
10	3050	crushed.

Specimen No. 24. Magnesian Limestone. Worksop. 1in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
13	3834	
14	3946	fractured.
...	......	....
38	7098	crushed.

Specimen No. 25. Sandstone. 1in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
8	2938	
9	3050	fractured.
...	......	....
13	3498	crushed.

Specimen No. 26. Sandstone. 2in. cube.

No. of Expt	Weight laid on in lbs.	Remarks.
11	9770	
12	10218	fractured.
...	......	....
20	12238	crushed.

Results of experiments on stone from North Wales and other places. Specimens Nos. 12, 14, 17, 18, 19, 20, 21, 22, 23, 24, 25, and 26.

No. of Specimen.	Description of Stone.	Locality.	Size.	Weight with which it fractured, in lbs.	Weight with which it crushed, in lbs.	Pressure required to crush a 2in. cube, in lbs.
12	Grauwacke.	Penmaenmawr ...	2in. cube.	40692	67572	67572
14	Granite.....	Mount Sorrel......	"	51444	51444	51444
17	Syenite.....	" "	"	47284	47284	47284
18	Granite.....	Bonaw, Inverary..	1¼in.cube	17396	24564	43669
19	"	Furnace, "	"	24564	24564	43669
20	"	(A)	"	22772	24564	43669
21	Limestone..	(B)	"	17396	19188	34112
22	"	(C)	"	18292	19188	34112
23	"	Anston........	1in. cube.	2154	3050	12200
24	"	Worksop............	"	3946	7098	28392
25	Sandstone..	... ...	"	3050	3498	13992
26	"	... ...	2in. cube.	10218	12228	12228

The Welsh specimen of grauwacke, from Penmaenmawr, exhibits great powers of resistance, nearly double that of some of the Yorkshire sandstones, and about one-third in excess of the granites, excepting only the granite from Mount Sorrel, which is to the Welsh grauwacke, as ·757 : 1. Some others, such as the Ingleton grauwacke, supported more than the granites, but are deficient when compared with that from Penmaenmawr. The specimen No. 23 is the stone of which the Houses of Parliament are built. Specimens Nos. 25 and 26 were broken to show experimentally the ratio of the powers of resistance as the size is changed. The results are sufficiently near to prove that the crushing weights are as the areas of the surface subjected to pressure.

The specific gravity and porosity of the different kinds of rock vary greatly, and Mr. Shaw, in his desire to obtain the best quality of Yorkshire paving stone, had those from the neighbourhood of Bradford carefully tested in regard to their powers of absorption; the experiments, which were conducted with great precision, gave the following results.

Experiments to ascertain the amount of water absorbed by various kinds of stone.

No. of Specimen.	Description of Stone.	Locality.	Weight before immersion.	Weight after immersion for 48 hours.	Diff'rence of Weight.	Proportion absorbed.
			lbs.			
1	Sandstone..	Shipley	5.4687	5.5546	.0859	1 in 63.6
2	"	Heaton..........	5.2578	5.3632	.1054	1 in 49.8
3	"	Heaton Park.	5.1718	5.2896	.1171	1 in 44.1
4	"	Spinkwell	5.2968	5.4726	.1758	1 in 30.1
5	"	Idle Quarry.	5.7178	5.8203	.1016	1 in 56.3
6	"	Jegrum's-lane	5.5976	5.7187	.1211	1 in 46.2
7	"	Spinkwell	5.6757	5.7851	.1094	1 in 53.8
8	"	Coppy Quarry......	5 5703	5.6914	.1211	1 in 46.0
9	"	Old Whatley..	5.4726	5.6132	.1406	1 in 38.9
10	"	Manningham-lane.	5.4882	5.6093	.1211	1 in 46.3
11	"	" "	5.6289	5.7539	.1250	1 in 45.0
12	Grauwacke.	Wales	6.4101	6.4140	.0039	1 in 1641.0
13	Granite.....	Mount Sorrel	5.6875	5.6992	.0117	1 in 485.0
14	"	" "	5.8007	5.8124	.0117	1 in 495.0
15	Grauwacke.	Ingleton.	5.75.00	5.7539	.0039	1 in 1962.6

From the above table it will be observed that specimen No. 15, the Ingleton grauwacke, is the least absorbent, and No. 12, the Welsh grauwacke, absorbs almost as little, while Nos. 9 and 14 of the sandstones absorb most. The granites, though closely granulated, take up much more water than the grauwackes, but less than the sandstones. The resistance of the grauwacke specimens to the admission of water is four times that of the granite, and thirty-six times that of sandstone, such as is found in the Yorkshire quarries.

No. of Specimen	Description of Stone.	Locality.	Size.	Specific gravity.	Pressure to fracture specimen.	Pressure to crush specimen.	Pressure per square inch to crush specimen.	Cubic feet in a ton.	Ratios of powers of absorption.
			cube		lbs.	lbs.	lbs.		1 in
1	Sandstone.	Shipley*	2in.	2.452	33524	38900	9725	14.616	63.6
2	,,	Heaton*	,,	2.420	33524	40692	10173	14.809	49.8
3	,,	Heaton Park* ..	,,	2.385	29940	31732	7933	15.027	44.1
4	,,	Spinkwell	,,	2.329	defec	tive.	....	15.388	30.1
5	,,	Idle Quarry†	,,	2.464	42484	43380	10845	14.545	56.3
6	,,	Jegrum's-lane† ..	,,	2.400	45172	47860	11965	14.933	46.2
7	,,	Spinkwell†	,,	2.456	31732	37108	9277	14.592	53.8
8	,,	Coppy Quarry† ..	,,	2.408	37108	41588	10397	14.833	46.0
9	,,	Old Whatley* ...	,,	2.415	35316	35316	8829	14.840	38.9
10	,,	Manning'm-lane*	,,	2.401	28148	37108	9277	14.927	46.3
11	,,	,, ,,	,,	2.421	fai	led.	....	14.804	45.0
12	Grauwacke	Penmaenmawr ..	,,	2.748	40692	67572	16893	13.042	1641.0
13	Granite ..	Mount Sorrel ...	,,	2.657	....	....	....	13.489	485.3
14	,,	,, ,, ...	,,	2.675	51444	51444	12861	13.398	495.0
15	Grauwacke	Ingleton	,,	2.787	35316	(53236)	not crd	12.866	1962.6
16	Granite ..	Aberdeen	,,	—	27546	(28340)	not crd	—	—
17	Syenite ..	Mount Sorrell ...	,,	—	47284	47284	11821	—	—
18	Granite ..	Bonaw	1½in	—	17396	24564	10917	—	—
19	,,	Furnace	,,	—	24564	24564	10917	—	—
20	,,	A	,,	—	22772	24564	10917	—	—
21	Limestone.	B	,,	—	17396	19188	8528	—	—
22	,,	C	,,	—	18292	19188	8528	—	—
23	,,	Anston..........	1in.	—	2154	3050	3050	—	—
24	,,	Worksop	,,	—	3946	7098	7098	—	—
25	Sandstone.	D	,,	—	3050	3498	3498	—	—
26	,,	E	2in.	—	10218	12228	3057	—	—

On comparing the results of the experiments on the Yorkshire sandstones, it will be seen that the difference of resistance to pressure does not arise so much from the variable character of the stone in different quarries, as from the position in which it is placed as regards its laminated surface, the difference being as 10:8 in favour of the stone being crushed upon its bed to the same when crushed in the line of cleavage; the same may be said of the limestones.

Comparing the strengths indicated by the above experiments, I find a very close approximation in the granites, but considerable difference in the Yorkshire sandstones. Mr. Rennie obtained his specimens from the same district, the valley of the Aire; but the force required to crush the Brom-

* Pressure applied in the direction of the cleavage.

† Pressure applied perpendicularly on the bed of the stone.

ley Fall stone was much less than that required to fracture similar specimens from the Shipley quarries. The following table gives some useful results for comparison.

Description of Material.	Crushing force in lbs. per square inch.	Authority.
Porphyry	40416	Gauthey.
Granite, Aberdeen	11209	Rennie.
„ mean of 3 varieties	11564	Experiments 14, 18, 19.
Sandstone, Yorkshire	6127	Rennie.
„ „ mean of 9...	9824	Experiments 1 to 9.
Brick, hard...	1888	Rennie.
„ red	805	Rennie.

From the above it is evident that there is a considerable difference between the results of Mr. Rennie's experiments and those in the preceding tables. This may, perhaps, be due to the different methods pursued in the experiments, or from taking the first appearance of fracture as the ultimate power of resistance. Whereas, there is in some cases a difference of nearly a third between the weight required to produce the first crack, and that required subsequently to crush the specimen. This is the more remarkable as all the specimens did not appear to follow the same law, as in some the weight which fractured the specimen by a continuation of the process ultimately crushed it. Experiments of this kind require close observation, and the reason just given may probably account for the difference between Mr. Rennie's and my own results.

All information respecting the strength of materials must be derived from direct experiment, which is always the safest and best guide; and fully aware of the importance of this fact, I have deemed it expedient to append the following list of the bearing powers of some other materials employed in building, and to which reference may be made in any case where the load is excessive, or where the material is subjected to severe strain.

The necessity of these experiments was the more apparent some years since, in the construction of the Britannia and

Conway tubular bridges, when fears were entertained of the security of the masonry to support, upon the given area, the immense weight of the tubes, upwards of 1,500 tons, resting on one side of the tower. To ascertain how far the material (Anglesea limestone) was calculated to sustain this load, the following experiments were instituted by Mr. Latimer Clarke.

"*Results of experiments made with actual weight on the materials used in the Britannia bridge, January,* 1848.

BRICKWORK.

	lbs. per square inch.
No. 1.—9in. cube of cemented brickwork (Nowell and Co.), No. 1 (or best quality) weighing 54 lbs., set between deal boards. Crushed with 19 tons 18 cwt. 2 qrs. 22 lbs.	= 551.3
No. 2.—9in. cube of brickwork, No. 1 weighing 53 lbs., set in cement. Crushed with 22 tons 3 cwt. 0 qr. 17 lbs.	= 612.7
No. 3.—9in. cube of brickwork, No. 3 weighing 52 lbs., set in cement. Crushed with 16 tons 8 cwt. 2 qrs. 8 lbs.	= 454.3
No. 4.—9¼in. brickwork, No. 4 weighing 55½ lbs., set in cement. Crushed with 21 tons 14 cwt. 1 qr. 17 lbs.	= 568.5
No. 5.—9in. brickwork, No. 4 weighing 54¼ lbs., set between boards. Crushed with 15 tons 2 cwt. 0 qr. 12 lbs.	= 417.
Mean	521.

Note.—The last three cubes of common brick continued to support the weight, although cracked in all directions; they fell to pieces when the load was removed. All the brickwork began to show irregular cracks a considerable time before it gave way.

The average weight supported by these bricks was 33.5

tons per square foot, equal to a column 583.69 feet high, of such brickwork.

Sandstone.

	lbs. per square inch.
No. 6.—3in. cube red sandstone, weighing 1 lb. 14⅝ oz., set between boards (made quite dry by being kept in an inhabited room). Crushed with 8 tons 4 cwt. 0 qr. 19 lbs..................	= 2043.
No. 7.—3in. cube sandstone, weighing 1 lb. 14 ozs., set in cement (moderately damp). Crushed with 5 tons 3 cwt. 1 qr. 1 lb.....................	= 1285.
No. 8.—3in. sandstone, weighing 1 lb. 15½ ozs., set in cement (made very wet). Crushed with 4 tons 7 cwt. 0 qr. 21 lbs........................	= 1085.
No. 9.—6in. cube sandstone, weighing 18 lbs., set in cement. Crushed with 63 tons 1 cwt. 2 qrs. 6 lbs.	= 3924.8
No. 10.—9½in. cube sandstone, weighing 58½ lbs., set in cement (77½ tons were placed upon this without effect, = 2042 lbs. per square inch, which was as much as the machine would carry)	
Mean............................	2185.

All the sandstones gave way *suddenly*, and without any previous cracking or warning. The 3in. cubes appeared of ordinary description; the 6in. was fine grained, and appeared tough and of superior quality. After fracture the upper part generally retained the form of an inverted square pyramid about 2½in. high and very symmetrical, the sides bulging away in pieces all round. The average weight of this material was 130 lbs. 10 ozs. per cube foot, or 17 feet per ton.

The average weight required to crush this sandstone is 134 tons per square foot, equal to a column 2351 feet high of such sandstone.

LIMESTONE.

	lbs. per square inch.
No. 11.—3in. cube Anglesea limestone, weighing 2 lbs. 10 ozs., set between boards. Crushed with 26 tons 11 cwt. 3 qrs. 9 lbs.	= 6618.
This stone formed numerous cracks and splinters all round, and was considered crushed; but on removing the weight about two-thirds of its area were found uninjured.	
No. 12.—3in. limestone, weighing 2 lbs. 9 ozs., set between deal boards. Crushed with 32 tons 6 cwt. 0 qr. 1 lb....................................	= 8039.
This stone also began to splinter externally with 25 tons (or 6220 lbs. per square inch), but ultimately bore as above.	
No. 13.—3in. limestone, weighing 2 lbs. 9 ozs., set in deal boards. Crushed with 30 tons 18 cwt. 3 qrs. 24 lbs.	= 7702.6
No. 14.—Three separate 1in. cubes limestone, weighing 2 lbs. 9 ozs., set in deal boards. Crushed with 9 tons 7 cwt. 1 qr. 14 lbs.	= 6995.3
All crushed simultaneously.	
Mean..............................	7579.

All the limestones formed *perpendicular* cracks and splinters a long time before they crushed.

Weight of the material from above = 165 lbs. 5 ozs. per cubic foot, or 13½ feet per ton.

The weight required to crush this limestone is 471.15 tons per square foot, equal to a column 6433 feet high of such material."

Previously to the experiments just recorded, it was deemed advisable not to trust to the resisting powers of the material of which the towers of either bridge were composed; and, to make security doubly sure, it was ultimately arranged to rest

the tubes upon horizontal and transverse beams of great strength, and by increasing the area subject to compression, the splitting or crushing of the masonry might be prevented. This was done with great care, and the result is the present stability of those important structures.

In conclusion, I have now to submit to the consideration of the society and the practical builder the following general summary of results, obtained from various materials, showing their respective powers of resistance to forces tending to crush them.

GENERAL SUMMARY OF RESULTS ON COMPRESSION.

	Description of Material.	Crushing force in lbs. per square inch.	Authority.
Iron and Steel.	Cast steel		Fairbairn's Experiments on the Mechanical Properties of Metals.—Transactions of the British Association, 1854.
	Blister steel		
	Cast iron (white, derived from 14 meltings)	214816	
	Ditto (from 12 meltings)	163744	
	Ditto (from ordinary castings)	89600	
Stone.	Porphyry	40416	Gauthey.
	Grauwacke, Penmaenmawr	16893	Exprmts. No. 12.
	Granite, mean of 3	11565	Do. Nos. 14, 18, 19.
	Sandstone, Yorkshire	6127	Rennie.
	Ditto, mean of 9 exprts.	9824	Exprmts. 1 to 10.
	Ditto, Runcorn	2185	Clark.
	Limestone	8528	Exprmts. 21, 22.
	Ditto, Anglesey	7579	Clark.
	Ditto, Magnesian—mean	5074	Exprmts. 23, 24.
	Brick, hard	1888	Rennie.
	Ditto, red	805	"
	Ditto, mean of 4 exprts.	1424	Clark.
Timber.	Box	9771	Hodgkinson.
	English Oak (dried)	9509	
	Ash (ditto)	9363	
	Plumtree (ditto)	8241	
	Beech	6402	
	Red Deal	5748	
	Cedar	5674	
	Yellow Pine	5375	

It is observed by Professor Hodgkinson, in his experiments on timber, that great discrepancies occurred when the woods were in different degrees of dryness; wet timber, though felled for a considerable time, bearing in some instances less than one-half what it bore when dry.

Professor Hodgkinson has also experimented on round and square columns of sandstone from Ped Delph, Littleborough, Lancashire, a much harder stone than that found on the banks of the Aire. With regard to these experiments, it appears "that there is a falling off in strength in all columns from the shortest to the longest, but that the diminution is so small, when the height of the column is not greater than about twelve times the side of its square, that the strength may be considered uniform, the mean being 10,000 lbs. per square inch or upwards.

"From the experiments on the columns 1in. square, it appears that when the height is fifteen times the side of the square, the strength is slightly reduced; when the height is twenty-four times the height of the base, the falling off is from 138 to 96 nearly; when it is thirty times the base, the strength is reduced from 138 to 75; and when it is forty times the base, the strength is reduced to 52, or to little more than one-third. These numbers will be modified to some extent by experiments now in progress.

"As long columns always give way first at the ends, showing that part to be weakest, we might economize the material by making the areas of the ends greater than that of the middle, increasing the strength from the middle both ways towards the ends. If the areas of the ends be to the area of the middle, as the strength of a short column is to that of a long one, we should have for a column, whose height was twenty-four times the breadth, the areas of the ends and middle as 13766 to 9595 nearly. This, however, would make the ends somewhat too strong,

since the weakness of the long columns arises from their flexure.

"Another mode of increasing the strength would be that of preventing flexure, by increasing the dimensions of the middle.

"From the experiments it would appear that the Grecian columns, which seldom had their length more than about ten times the diameter, were nearly of the form capable of bearing the greatest weight when their shafts were uniform, and that columns tapering from the bottom to the top were only capable of bearing weights due to the smallest part of their section, though the larger end might serve to prevent lateral thrusts. This latter remark applies, too, to the Egyptian columns, the strength of the column being only that of the smallest part of the section.

"From the two series of experiments, it appeared that the strength of a short column was nearly in proportion to the area of the section, though the strength of a larger one is somewhat less than in that proportion."

I give these extracts from Mr. Hodgkinson's paper, to show the advantages to be derived from proper attention to the construction of columns, not only as regards their resistance to a crushing force, but as to the propriety of enlarging the ends to increase their powers of resistance.

Experimental data cannot always be applied in architectural constructions; but it is, nevertheless, essential that the architect and builder should be cognizant of the facts, in order that they may prepare their plans, as far as possible, in accordance with them, and effect the greatest amount of work with the least waste of material.

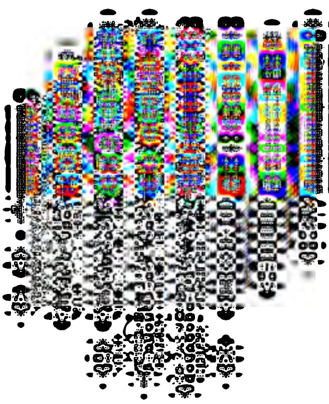
The accompanying plate exhibits the appearance of some of the fractured specimens; in all, it will be observed, that there is a tendency to give way by one or more wedges forcing out from the sides in all directions.

The plate shows the fractured appearance of,

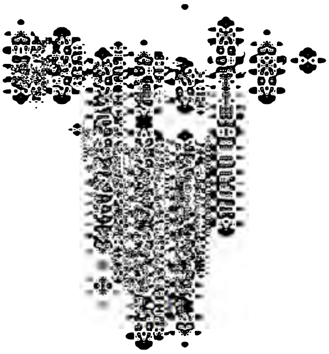
Specimen No.	1.	Sandstone,	Shipley, Bradford.
,,	2.	,,	Heaton, Bradford.
,,	3.	,,	Heaton Park, Bradford.
,,	5.	,,	Idle Quarry.
,,	7.	,,	Spinkwell.
,,	12.	Grauwacke, Penmaenmawr, Wales.	
,,	14.	Granite, Mount Sorrel.	
,,	20.	,,	A.
,,	21.	Limestone,	B.
,,	22.	,,	C.
,,	23.	,,	Anston.
,,	25.	Sandstone,	D.
,,	26.	,,	E.

The other specimens operated upon were not sufficiently defined in the line of fracture to admit of their form being sketched; most of them having been crushed almost to powder.

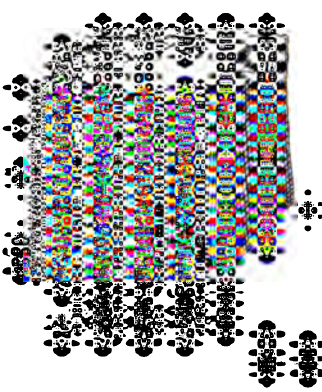
Specimen No 2

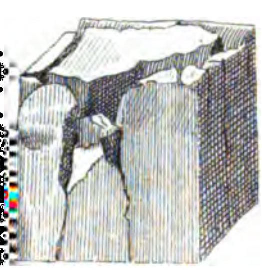


Heaton



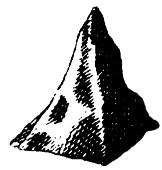
Specimen No 5





Idle Quarry

Specimen No. 20



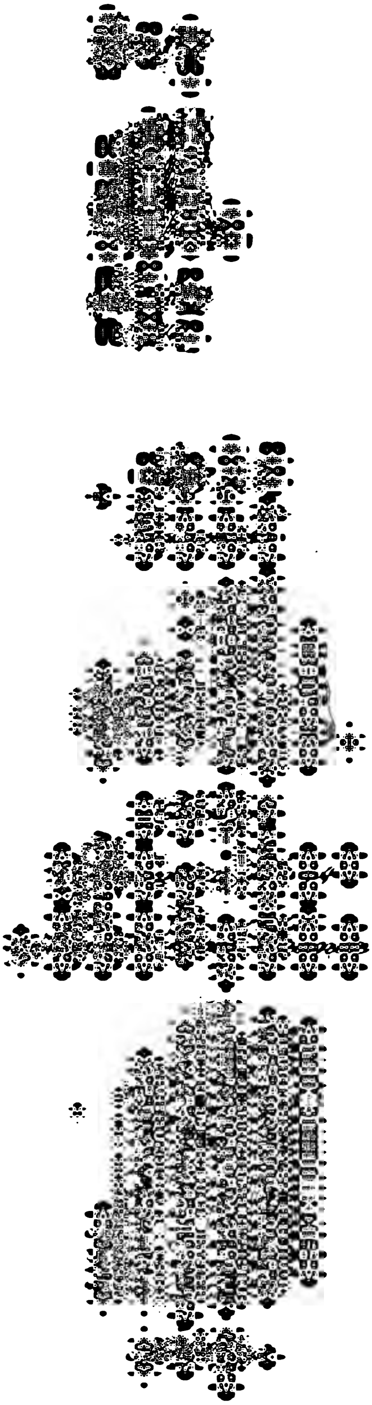
A

Granite

Specimen No. 22

C

Limestone



D

Sandstone
No. 25
1 Cube

E

...stone No. 26
2 Cube

IV.—*On the Fusion of Metals by Voltaic Electricity.*

By J. P. Joule, F.R.S., &c.

[*Read March 4th,* 1856.]

The attention of practical scientific men has of late been much occupied with the question, how far it is possible to forge large masses of iron, without destroying the tenacity and other valuable qualities of the metal employed. In welding iron, the metal is raised to the high temperature at which it assumes a soft and incipient viscid consistency. Two pieces of iron in this condition will adhere together slightly, if merely placed in contact with one another. That a firm junction cannot be made in this way is simply owing to the fact that few particles are brought into contact, and that the metallic continuity is only established at those points. The hammer is, therefore, employed to cause the entire surfaces to meet together. The same end has also been attained by the employment of great pressure; and probably we shall ultimately see large masses of forged iron formed by simply subjecting a bundle of smaller pieces, raised to the welding temperature, to the operation of great pressure. To succeed in the latter process, it would however be requisite to press clean unoxidized surfaces together. Indeed the importance of presenting clean surfaces together in ordinary welding cannot be too strongly insisted on; for, if oxide of iron be present, a portion of it will not fail to remain at, or in

the neighbourhood of, the juncture, and seriously impair the quality of the iron at those points.

It occurred to me some months ago that it might be possible to employ the calorific agency of the electric current in the working of metals. By the use of a voltaic battery there appeared to be no doubt but that small pieces of metal could be fused into one lump. If so, it was obvious that by employing a battery of adequate size the largest masses of wrought iron could be produced, the question resolving itself simply into one of cost. It was not before the last month that I had an opportunity of witnessing an experiment on a small scale. It was performed in the laboratory of Professor Thomson. He surrounded a bundle of iron wires with charcoal, and, after transmitting a powerful current through it for some time, the wires were found in one part to be completely fused together.

More recently I have made several experiments in which the wires were placed in glass tubes, surrounded with charcoal, &c. With a battery of six Daniell's cells I have thus succeeded in fusing several steel wires into one, uniting steel wires with brass, platina with iron, &c. I doubt not but that in many instances the process would advantageously supersede that of soldering, especially when, for thermo-electric or other purposes, it is desirable to join metals of difficult fusibility without the intervention of another metal which melts at a lower temperature.

Having demonstrated the possibility of obtaining perfect junctions by means of the voltaic current, let us inquire what expenditure of battery materials would be necessarily involved. In the outset it may be remarked that were not heat continually removed, by conduction, convection, and radiation, from a wire carrying a current of however low a degree of intensity, the wire would ultimately attain an excessively high temperature on account of the continuous augmentation

of heat within it. Now the escape of heat may be largely prevented by means of non-conducting substances, and will be nearly proportional to the surfaces, so that by employing sufficiently large masses of metal, and surrounding them with non-conducting materials, it may be reduced to almost any extent. The quantity of zinc required to fuse a large mass of iron may therefore be estimated as follows.—

I have shown in a paper already communicated to the Society, that the quantity of heat due to the intensity of a Daniell's cell is 6°.129 per pound of water for every 33 grains of zinc dissolved.* In working with a voltaic battery, it is generally an advantageous arrangement to make the resistance of the battery one-half that of the entire circuit. Hence, as the quantity of heat evolved in any part of the circuit is proportional to its resistance, we may take half the above, or 3°.064 per pound of water, as the heat which may be advantageously produced outside a Daniell's battery by the dissolution of 33 grains of zinc. Calling the temperature of incipient fusion of iron 4,000° above the ordinary temperature of the atmosphere and the specific heat of iron 0.11, we find 4,740 grains of zinc to be the quantity consumed in the voltaic battery, in order to raise one pound of iron to the temperature of fusion. But as a considerable quantity of heat will be rendered "latent," 5,000 grains may be taken as the estimate of minimum consumption of zinc, in a Daniell's battery, in order to effect the fusion of one pound of iron.

The same effect is due to the heat evolved by the combustion of 500 grains of coal, but on account of the large quantity of heat which must necessarily escape up the chimney of a hot furnace, we may estimate the minimum actual consumption at 1,000 grains.

The quantity of zinc consumed in the voltaic process is therefore nearly equal to that of the iron to be melted, but it would

* Memoirs of the Literary and Philosophical Society, Vol. vii., p. 94.

be possible to effect the same object in a more economical manner, by availing ourselves of the use of the magneto-electrical machine. This machine enables us to obtain heat from ordinary mechanical force, which mechanical force may again be derived from the conversion of heat, as in the steam engine. In a steam engine it is practically possible to convert at least one-fifth of the heat due to the combustion of coal into force, and one-half of this force applied to work a magneto-electrical machine may be evolved in the shape of heat. Hence then it is possible to arrange machinery so as to produce currents of electricity which shall evolve one-tenth of the quantity of heat due to the combustion of the coal employed. So that 5,000 grains of coal used in this way would suffice for the fusion of one pound of iron.

V.—*A Short Account of the Life and Writings of the late Mr. William Sturgeon.*

By J. P. Joule, V.P., F.R.S., Hon. Mem. Phil. Soc. Cambridge, Corr. Mem. R.A. Turin, &c.

[*Read October 7th,* 1856.]

A pardonable vanity of our human nature makes us seek into the past, in order to connect the names of those we respect and love, with the great and good of ancient times. In so doing we award to our contemporaries a portion of the praise and admiration which are due to their eminent forefathers. But the search is often unprofitable, and many of our greatest men, descended from an obscure or unknown parentage, have been the first to render the family name illustrious. Of such was the subject of the present memoir, who was born in 1783, at Whittington, a village in the county of Lancaster, near the border which divides it from Westmoreland. His father was John Sturgeon, a shoemaker, who came from the neighbourhood of Dumfries, and married Betsy Adcock, daughter of a small shopkeeper in Whittington. The offspring of this marriage were William, Margaret, and Mary. Of these, Mary died in infancy; and Margaret married John Coates, and died the mother of a numerous family.*

John Sturgeon appears to have been a man who paid more attention to fishing for salmon in the river Lune, a pursuit in

* The youngest of these, Ellen, who was only a few weeks old at the time of her mother's death, was on that event kindly taken into the family of her uncle, William Sturgeon, and ever afterwards treated as his own daughter.

which he gained much local celebrity, than to his legitimate business. Young William at a tender age was nightly compelled, with a torch in his hand, to wade up to his middle in the water, while his father stood ready to spear such fish as were attracted by the glare. The treatment he experienced during the day was equally harsh, and he was liable to severe chastisement whenever his strength proved unequal to labours which would have been unsuited to a child of double his age. It argues strongly for his domestic virtues and filial piety that in after years the happiness of this parent, to whom he owed so little, was the object of his constant solicitude. Nor were these efforts lost. His father's old age bore the fruits of virtue and religion.

At ten he had the misfortune to lose his mother, an estimable woman, upon whom, in consequence of the habits of his father, the support of the family mainly depended. Apprenticed, at the age of thirteen, to a shoemaker at Old Hutton, a village near Kirby Lonsdale, he was doomed to be even more cruelly treated than at home. The ill usage of apprentices was very common at that time, and the perpetrators of it too frequently escaped with impunity. Not content with exacting a slavish drudgery on the week day, this man compelled Sturgeon on the Sunday to carry game cocks from one part of the country to another, so as, in the language of that ancient but detestable sport, to change their walks. He also used much violence, and starved his apprentices to such a degree that they were obliged to make free with the hares and rabbits of the neighbourhood in order to supply their hunger. So long as Sturgeon was a mere child he patiently submitted to this tyranny, but when at the age of sixteen he felt strong enough to resist force by force, he apprised his master of that circumstance, who, with the low cunning and cowardice suitable to such a character, ever afterwards observed a more conciliatory deportment.

It seems almost incredible that, amid such hardships and surrounded by such vicious examples, Sturgeon could have acquired anything valuable beyond mere proficiency in his trade, or that he could even have resisted the temptation to follow evil courses so constantly held out. But it, nevertheless, appears that he contrived to acquire some proficiency in music, and the knowledge of several mechanical arts. His ability in the exercise of one of the latter may be judged by the following anecdote, which will illustrate at the same time the energy of character which became so conspicuous in after life. One of the celebrations of the Guild at Preston took place during his apprenticeship, and William Sturgeon and his fellow-apprentice having heard much talk about it, determined to go and see it. Their master they well knew would not give his consent, but this difficulty they got over by the simple expedient of starting on their trip without asking it. Another and more serious difficulty arose from the awkward circumstance of neither of them possessing any money. Sturgeon, who had shown some talent in cleaning clocks and watches during his residence at Hutton, contrived not only to support himself and fellow-apprentice during their journey to and from Preston by what he obtained from clock cleaning by the way, but even to arrive at home with money in pocket. On their return from satisfying their curiosity at Preston Guild, the apprentices had to undergo the ordeal of a sound thrashing from their brutal master.

Dissatisfied with his position as a journeyman shoemaker, and seeing little prospect of improving his condition at Kirby Lonsdale, he, in 1802, enlisted in the Westmoreland Militia, and two years afterwards volunteered as a private into the 2nd Battalion of Royal Artillery. Soon after entering this corps he married Mary Hutton, a widow, who kept a shoe shop at Woolwich. The issue of this marriage was two daughters and a son, namely, John and Elizabeth, twins, and Ann, all of whom died in their infancy. After the death of

his first wife he married, in 1829, Mary Bromley, of Shrewsbury, by whom he had a daughter, who died in infancy.

Being called into foreign service soon after his marriage with his first wife, he made the first systematic efforts for the improvement of his mind. While in Newfoundland he was first directed to the contemplation of electrical phenomena, on the occasion of a terrific thunderstorm. His interest in the works of nature being thus excited, he prepared himself for their study, by teaching himself reading and writing and the elements of grammar. A sergeant in the artillery possessed a tolerable library of books, to which the generous owner gave him constant access. It was his practice when he came off guard at night to take from his knapsack the book and candle he generally carried there, and to spend that time in reading which by the other soldiers was devoted to repose. He was thus able to devote a considerable time to mathematics, and to the study of the dead and living languages. To these he added optics, and various other branches of natural philosophy. He also found time to acquire various mechanical arts, such as that of lithography, in which he attained afterwards considerable proficiency.

Notwithstanding his love of these pursuits he did not neglect, while in the army, his trade as a shoemaker, and the excellence of his workmanship caused him to be employed in this capacity by most of the officers of the corps. After leaving the army he followed this avocation in Lancaster, until, at the instance of his wife, whose relations resided at Woolwich, he returned thither, and began to prosecute with renewed assiduity his favourite intellectual studies.

The difficulties he encountered in his strivings after scientific truth at this time, will be best understood by quoting his own words from a manuscript I have by me. He says:—"After leaving the service of the Royal Artillery in 1820, and as my slender finances presented opportunities, I turned my attention to scientific inquiries, and to the construction of some of those

philosophical apparatus which, from my boyhood, I had been an admirer of, but which, during *my* services at least, a military profession precluded all further knowledge of than that which was to be derived from books of science; a species of commodity at that time exceedingly rare in the army, even in the distinguished branch to which I had the honor to belong. Fortunately for the service, every encouragement is now given for mental improvement both in the army and navy. Having spent the prime of life in the service, during a period of the most rigorous military discipline that ever occurred to the British army, I can vouch from experience that the value of a soldier is not likely to be deteriorated by mental cultivation; nor do I think it possible that any soldier could be more devoted to his profession, than he who improves his mind by scientific studies. The more science is cultivated in the service the more efficient will be its members in the performance of their respective duties, and consequently the more formidable will they be found when those duties are required against a well disciplined army."

"My first step towards constructing apparatus was the purchase of an old lathe, and a few tools to work with. My stock of tools, however, was very limited, from a want of means to purchase more; so that many rude substitutes had frequently to be brought into requisition. And as I had no practice either at the lathe or the vice, my first essays in apparatus making were necessarily deficient of those elegancies of structure and refinements of workmanship, which always characterize the productions of regular-bred instrument makers."

"Notwithstanding these disadvantages, however, *hope* and perseverance surmounted every difficulty that was met with, and I soon found myself capable of constructing such pieces of apparatus as I immediately wanted for my own purpose; and it was not long before I found purchasers for others, even amongst the first-rate instrument makers in London. I am

not aware that my progress was due to any particular genius that I possessed, although I never had even a moment's instruction from any person; and I record these particulars with no other view than that of encouraging others to persevere, who may have similar disadvantages to encounter."

"Having become acquainted with Mr. Thomas Rose, a gentleman of considerable scientific attainments, and at that time (1823) residing in Woolwich, I had the good fortune to become possessed of an electrical machine, much earlier than I had expected. Mr. Rose being about to construct a machine of this kind for his own use, presented me with the materials he had prepared for it as soon as he became aware that I was in want of one. By the liberality of this gentleman, I had thus placed in my hands a glass cylinder ten inches in diameter, and an excellent mahogany frame for its support. Both parts were quite new, but the cylinder was not mounted, nor was it furnished with caps at its necks. Although the fitting up of this machine and its appendages cost a considerable portion of time, I was amply rewarded by its happening to be an excellent *working* apparatus, which I used in my lectures and investigations for several years afterwards."

As above intimated, Mr. Sturgeon procured his discharge from military duty in 1820, his conduct while a soldier having been, according to the testimony of his commanding officer, Major Jones, "altogether unimpeachable," and was thus enabled to devote himself with greater assiduity to the pursuit of his favorite studies. At that period scientific research had just received a great impulse from the discovery by Oersted, that a current of voltaic electricity passing through a conducting wire, is capable of deflecting a poized magnetic needle from its position in the magnetic meridian of the earth. This most remarkable discovery, by which the sciences of electricity and magnetism were permanently allied, created an intense interest, and accordingly we find that the most eminent natural philosophers of the day immediately entered upon the

new field of investigation thereby opened. Davy and Arago magnetized steel needles, by placing them in the vicinity of a wire carrying a voltaic current. Ampère found that two parallel wires conveying currents in the same direction attracted one another, but that they repelled one another if the electricity travelled in contrary directions; and Faraday produced the revolution of a magnetic bar round a conducting wire, as well as the reverse phenomenon of the revolution of a conducting wire round one of the poles of a magnet.

Mr. Sturgeon followed in the same track, and the mechanical skill he possessed enabled him to construct and improve the beautiful revolving apparatus, which had been devised by Faraday, Barlow, Ampère, and others. The rotations are in all cases owing to the action of a wire carrying a current of electricity, upon either a permanent magnet or another electro-dynamic wire, and may in every case be traced to Oersted's law. Considerable instruction may, however, be derived from their study, and it would appear that they were the object of Mr. Sturgeon's earliest scientific efforts. We accordingly find in the *London Philosophical Magazine* for September, 1823, an account of his modification of Ampère's rotating cylinders. This contrivance is succinctly described by Mr. Jones, optician, as "consisting of two sets of revolving cylinders, one suspended on each pole of an inverted horse-shoe magnet. Upon the insertion of dilute nitric acid the two sets of cylinders simultaneously enter into rotations, in a very interesting and striking manner." The effect, adds the same authority, is the most pleasing I have ever seen.

In 1824 Mr. Sturgeon published four papers on thermo-electricity, in which he succeeded in giving a fresh proof of the complete analogy which subsists between thermo-electric and voltaic currents.

In 1825 he presented to the Society of Arts the *complete set of novel electro-magnetic apparatus*, which is described in their transactions. The great merit of this collection con-

sisted in the improved adaptation of the batteries, magnets, &c. to one another, by means of which he was enabled to perform with a galvanic arrangement, no larger than a pint pot, experiments which had previously required the use of a cumbrous and costly battery. A principal improvement was effected by increasing the size and power of the magnet, Mr. Sturgeon having had the sagacity to observe that the intensity of the action was more advantageously augmented by an increase of the magnetic force, than by an enlargement of the size of the conductors of electricity, whereby the friction on the pivots of the revolving apparatus would have been made too sensible. The Society of Arts testified its sense of the importance of this contribution, by awarding to its author its large silver medal, with a purse of thirty guineas.

The most important piece of apparatus in the above collection was a bent bar of iron, surrounded with a coil of conducting wire. It was the earliest contrivance for showing the extraordinary and instantaneous inductive effect of a voltaic current on soft iron. I have already adverted to the magnetization of steel needles by Arago and Davy, but it appears that Mr. Sturgeon was the first who observed the wonderful facility with which soft iron can be made intensely magnetic by the galvanic current, as well as the extreme rapidity of the action. Mr. Sturgeon appears to have discovered the soft iron electromagnet, and to have constructed it both in the straight and horse-shoe shape as early as 1823. The soft iron electromagnet has been since introduced into most electric telegraphs.

A paper published in the *Philosophical Magazine* for June, 1826, contains a discussion of the cause of difficulty of firing gunpowder by electrical discharges. Mr. Sturgeon showed that in order to succeed, it was necessary to place a body of low conductive power in the circuit, such as a piece of wet thread. The violence of the discharge was by this means so much diminished, that the electric fluid from eight feet of

coated glass produced no sensible shock when passed through the body, a burning sensation only being experienced. He remarks, that "by employing either a moistened thread, or narrow tube of water, gunpowder may be ignited at several interruptions of the same circuit. I have frequently passed the fluid through my own body and fired six guns by one discharge of a jar, and so instantaneous is the ignition at the several guns, that their united reports appear like the report of one gun only."

In 1830, Mr. Sturgeon published a pamphlet, entitled, "Experimental Researches in Electro-Magnetism, Galvanism," &c., embracing an extensive series of experiments, chiefly in relation to the structure and action of galvanic batteries and the dry electric column.* It contains the record of a variety of observations, proving that the strongest chemical action on the positive plates was not always accompanied by the transmission of the most powerful currents of electricity. Mr. Sturgeon considered these experiments to militate against the chemical theory of galvanism; but a more searching investigation would have convinced him that, in the adduced instances, the chemical action was in a great measure occasioned by minute local circuits on the plates. It was reserved for Dr. Faraday, a year afterwards, to establish the chemical theory in an unassailable position, by his great discovery of the definite chemical action of electrical currents. There are, however, some facts in this pamphlet which have led to practical results of very great importance. I allude, first, to the remarkable change produced in the electrical powers of a metal by roughening its surface, such roughening being shown by Sturgeon to render zinc less electro-positive, so much so, that voltaic batteries constructed of no other metals than rolled zinc and cast zinc are "suffi-

* This work is unfortunately only a fragment. The printer died shortly after a portion was finished, leaving his affairs in a disordered state. The MS. of the remainder was consequently lost.

cient for the exhibition of most electro-magnetic experiments." Mr. Sturgeon was prepared to meet with this remarkable fact, having early in 1827 constructed several dry electric columns, in which zinc roughened on one side was the only metal employed. The importance of employing rolled instead of cast zinc for voltaic batteries was thus made evident.

The second fact to which I shall allude, as developed in this pamphlet, is the great advantage to be derived from amalgamating the zinc plates of a voltaic battery. Sir Humphrey Davy, in his Bakerian Lecture for 1826, announced that amalgamated zinc is positive relatively to pure zinc, without, however, drawing any conclusion regarding the construction of voltaic batteries. Mr. Kemp, in 1828, published in Professor Jameson's Journal, the account of a battery in which the positive metal consisted of liquid amalgam of zinc. Mr. Sturgeon appears, however, to have been the first person who introduced the use of amalgamated plates of zinc. After pointing out the method of amalgamating the plates, by first dipping them in a solution of sulphuric acid, and then immersing them in mercury, or spreading mercury over them with a piece of rag, he says, "were it not on account of the brittleness and other inconveniences occasioned by the incorporation of the mercury with the zinc, amalgamation of the surfaces of the zinc plates in galvanic batteries would become an important improvement; for the metal would last much longer, and remain bright for a considerable time, even for several successive hours,—essential considerations in the employment of this apparatus. Notwithstanding the inconveniences, however, the improvement afforded by amalgamating the surfaces of zinc plates becomes available in many experiments; for the violent and intense chemical action which is exercised on zinc by a solution of sulphuric or muriatic acid, with the consequent evolution of heat, and annoying liberation of hydrogen, have no place when the plates are amalgamated; the action is tranquil and uniform,

and the disengagement of gas, which is trifling, occurs only when the circuit is complete, and at the surface of the copper plate. The electric powers are highly exalted, and continue in play much longer than with pure zinc; and the only care of the experimenter is to prevent the copper, or whatever metal be substituted, from becoming amalgamated."

From the earlier portion of the above extract, it has been alleged that Mr. Sturgeon did not fully perceive the practical superiority of amalgamated, over ordinary zinc plates; but generous and even candid criticism will attribute his reserve to his praiseworthy aim to place before the reader the probable disadvantages as well as the real ascertained advantages of his invention. How much the latter predominated over the former, in Mr. Sturgeon's mind, will be obvious to any one who reads the whole passage. I need hardly remark, that amalgamated zinc plates are at the present day employed in Grove's, Daniell's, and, in short, every form of improved battery.

I next notice a memoir on the thermo-magnetism of homogeneous metals, published in the *Philosophical Magazine* for 1831, which contains a very laborious investigation of the effect of local heat on simple metals, by which he confirms Yelin's observation, that currents are produced in any metal by an unequal application of heat, and shows that these currents have a particular reference to the crystalline structure of the metal; bismuth, antimony, and zinc, which in a pure state exhibit the phenomena in an eminent degree, losing it nearly altogether when their crystalline condition has been destroyed by alliage with a small portion of tin or lead. Professor Thomson has recently discovered that *strain*, whether arising from crystalline structure or any other cause, produces, according to certain laws, an alteration both in the thermo-electric character of metals, and their power to conduct electrical currents.

In the year 1825 Arago announced his remarkable dis-

covery of the magnetism of rotation. He showed that a magnetic needle was deflected when a disc, composed of any kind of substance, was revolved near it. This very interesting phenomenon immediately occupied the attention of the most distinguished physicists, among whom the names of Seebeck, Herschel, Babbage, Christie, and Barlow, may be particularly mentioned. Herschel and Babbage remarked that a slit in the revolving plate materially diminished its action on the needle. Mr. Sturgeon took up the subject in 1832, and after much labour came to the conclusion that the effects were probably owing to a disturbance of the electric fluid by magnetic action. The words he uses are, "It would, however, be no great stretch of the imagination to suppose a disturbance of the electric fluid by magnetic action, as it would be only a kind of re-action to that which takes place in electro-magnetism." From the above extract there can be little doubt that Mr. Sturgeon would presently have arrived at the discovery of magnetic electricity, had he not been anticipated by Dr. Faraday, who, in November, 1831, communicated to the Royal Society his paper, "On the Evolution of Electricity from Magnetism," which forms the 2nd section of the first of that inimitable series of experimental researches, which has contributed so greatly to the scientific renown of Britain.

Van Marum and others had shown that a voltaic battery is able to charge coated glass. After repeating their experiments, Mr. Sturgeon constructed an apparatus, by means of which a leyden battery in connection with a series of Cruickshank's voltaic plates is repeatedly charged and discharged, by means of the revolution of a toothed wheel. In this way "the discharges can be made in such rapid succession as to prevent the sensation of distinct shocks, the effect on the animal economy being similar to that produced by a voltaic battery charged with acid and water. No shock was produced independently of the glass battery."

In 1836 Mr. Sturgeon communicated a paper to the Royal Society, which, however, being denied insertion in the *Philosophical Transactions*, received publication in the *Annals of Electricity*, entitled, "Researches in Electro-Dynamics." It contains the description of a machine, in which a coil of wire made to revolve between the poles of a horse-shoe magnet becomes the source of copious electrical currents. Another machine is also described, in which the revolving coil is furnished with a core of iron. Magneto-electrical coil machines had been constructed previously, the most celebrated being those of Pixii and Saxton. In these machines the current reciprocated at every half revolution of the coil, or a stronger current being forced in one direction than in another, the difference only could be utilized. Therefore, though able to exhibit some interesting phenomena, such as the spark and shock, they were almost powerless to produce deflections of the magnetic needle or electro-chemical decompositions. Mr. Sturgeon, by means of a very beautiful arrangement of four semi-wheels, mounted on the axis of his machine, united the entire energies of the opposing currents in one direction. This admirable invention received at his hands a still further improvement, by the introduction of springs pressing against the revolving discs, and the use of oil as a lubricating agent; mercury, which presents many very serious inconveniences, being thus superseded. In fact, the magneto-electrical machine received from Mr. Sturgeon's hands an improvement even more essential than that which the steam engine received from the genius of Watt, and henceforth it could be employed for telegraphic communication, chemical decomposition, and in short all the purposes for which a voltaic battery is available. An opinion is expressed by Mr. Sturgeon that the magneto-electrical machine thus improved, would ultimately entirely supersede the use of the voltaic battery, and I cannot but concur in it for reasons which I have stated fourteen years ago. The argument is simply this. It has been shown by

Liebig, and also demonstrated by my own experiments, that the production of force by the oxidation of zinc, is at least one hundred times as expensive as that derived from the combustion of coal in the steam engine. Therefore, admitting that half the effect is wasted during the conversion of ordinary force into that of current electricity, and also that half the useful effect is wasted in the production of force by the voltaic battery, we arrive at the conclusion that the zinc requisite to produce a current of given intensity is at least twenty-five times as expensive as the coal which, consumed in a steam engine working a Sturgeon's magneto-electrical machine, would be able to effect the same result.

In an inquiry on the "Attributes of the Galvanometer," he found that a voltaic pair, enfeebled by a long continued action, is able to produce a powerful temporary current after a few minutes rest. I have since been able to show that the strong initial current which takes place on the first immersion of a pair of zinc and copper plates into dilute acid is owing to the presence of oxygen on the surface of the negative plate.

In 1832 Mr. Sturgeon constructed an electro-magnetic engine for turning machinery, which was the first contrivance by means of which any considerable mechanical force was developed by the voltaic current. Since that time, engines of various forms have been constructed by Jacobi, Davidson, and others; but, as I have already observed, the hopes once entertained of superseding steam as a motive force were found to be fallacious.

Professor Henry appears to have been the first who observed a spark on the disruption of a voltaic circuit in which a long wire or coil was included. This phenomenon he justly attributed to the dynamical induction discovered by Dr. Faraday. Mr. Jenkin observed that on placing a bar of iron within the coil, the intensity of the electrical action on the breaking of the circuit is sufficient to produce a shock. Henry and Faraday extended the subject still further, by

papers published almost simultaneously in the beginning of 1835. In 1837 Mr. Sturgeon produced his electro-magnetic coil machine, consisting of a coil of thick wire surrounded by a helix of thin wire. The coil of thick wire was in connection with a pair of galvanic plates: an arrangement being made for rapidly making and breaking the circuit. The ends of the helix of thin wire were employed to communicate the shock, which is further enhanced by the insertion into the axis of the coil of a *bundle of iron wires*, or a *scroll of thin sheet iron*. This apparatus is the most efficient ever employed by medical practitioners, and its power is such that "a strong shock and bright spark are produced when the battery employed consists of a copper and zinc wire (No. 15) immersed one-tenth of an inch deep in dilute nitrous acid." Mr. Dancer has made a capital improvement on this instrument, by supplying a self-acting apparatus, consisting of a piece of iron attached to a spring, by the intermittent attraction of which the use of a toothed wheel is dispensed with.

A research concerning the fracture of leyden jars by electrical explosions conducted Mr. Sturgeon to the discovery of the means of preventing such accidents, by connecting the inner coating of the jar with the rod which supports the ball by means of strips of tinfoil. So effectual did Mr. Sturgeon find this simple contrivance to be, that during twelve years he did not break a single jar, although during that period he discharged a battery of twelve jars some hundreds of times from the most intense electrization.

In the autumn of 1838 a series of experiments was performed at the house of Mr. Gassiott, of Clapham-common, with a voltaic battery which was the joint property of Mr. Gassiott and Mr. Mason. These experiments appear to have been conducted by Messrs. Gassiott, Mason, Sturgeon, and Walker. After various interesting results had been obtained, Mr. Sturgeon, who had been previously requested to provide a catalogue of the experiments which it would be

desirable to make, had the opportunity of observing, for the first time, a highly important fact, which he describes in the following words:—"The battery was in series of one hundred and sixty pairs. I brought the tip ends of the polar wires (copper wire, one-tenth of an inch diameter) into contact, end to end, then withdrew them gently and very gradually from each other, keeping the flame in full play between them, till they were separated about one-fourth of an inch. In a few minutes the positive wire got red hot for half an inch, but the negative wire never became red. I repeated this several times in order to be convinced of the fact. I next laid the wires across one another, and brought them into contact about an inch from the extremities, and separated them as before. In a short time the whole of that part of the positive wire from the point of crossing to the extremity became very red hot, but the negative end never got even to a dull redness; it was certainly very hot, but never higher than a black heat. I next increased the length of the ends of the wires exterior to the circuit, and eventually heated two inches of the positive wire to a bright redness, but no such heat took place on the other wire. Thus satisfying myself that I was not mistaken, I called Mr. Mason to come and look at it; and, after satisfying that gentleman by an experiment or two, we called Mr. Gassiott and Mr. Walker to come and witness the novel phenomenon." Mr. Sturgeon considered that the heat was driven along bodily by the electric current, so as to accumulate at the point whence the discharge across the air takes place, an hypothesis which cannot be entertained at the present day. The true explanation of the phenomenon is most probably that advanced by Professor Thomson, viz., that in thermo-electric arrangement air stands in the same ratio to copper as bismuth does to antimony. Whence heat is evolved by electricity passing from copper to air, but cold by the reverse action.

Porrett had discovered a disturbance of the hydrostatic

level by voltaic action. In his experiments, a glass vessel divided into two compartments, by a bladder partition, had one of the partitions filled with water. By sending a voltaic current through the diaphragm the water was carried along with it until nearly the whole of it was driven into the other compartment. Mr. Porrett employed a battery of eighty cells, but Mr. Sturgeon succeeded in obtaining a striking result, of a similar nature, by the use of a simple Daniell's cell, the copper and zinc plates of which were connected together by a conducting wire; after a few days action the solution of sulphate of copper was found to be four inches higher than the level of the liquid in contact with the zinc plate. There is some reason to believe, that in Mr. Sturgeon's experiment, the ordinary phenomenon of endosmose and exosmose, so ably discussed by Professor Graham, had some place. Indeed, Mr. Porrett's experiment is, probably, only a peculiar instance of the mutual action of two dissimilar fluids in a porous material, the one fluid positively electrified, and the other negatively electrified water.

In investigating the action of heat on the poles of a magnet, Mr. Sturgeon found that an alteration of attraction for a suspended needle was thereby produced, which alteration subsided to a considerable extent as the magnet became restored to its original temperature. Mr. Sturgeon drew from his observations the conclusion that the magnetic poles move from the point where the heat is applied. Subsequently, on repeating the experiment by which Barlow found that the magnetic virtue of iron disappeared at a white heat, he found that a full red heat was capable of effecting the same result. His paper, entitled "Some Peculiarities in the Magnetism of Ferruginous Bodies," published in the seventh volume of the Society's Memoirs, contains an able discussion of this interesting subject; results of a very striking character being obtained with bars magnetized either by electric currents or the inductive action of the earth. Mr. Sturgeon

found that on heating one part of a bar to a full red heat, the remainder acted just as if the heated portion had been removed altogether.

Mr. Sturgeon had shown, in his "Experimental Researches in Electro-Magnetism and Galvanism," published in 1830, that when two similar plates of iron are placed, one in each of two strong solutions of nitric acid of different degrees of strength, having a bladder partition between them, they form an active voltaic pair: a battery of ten such pairs enabled him to decompose water and to ignite metals. At the same time he showed the high rank held by iron as an electro-negative element, when associated with amalgamated zinc. In 1836 Daniell observed that iron is sometimes more efficient than platinum, in association with amalgamated zinc; and about the same time Roberts and Fyfe recommended the employment of iron in the construction of voltaic batteries. In 1839 Mr. Sturgeon constructed a battery, consisting of twelve iron gas tubes, furnished with strips of amalgamated zinc, which proved very efficient. Afterwards, in 1840, he fitted up ten cylindrical jars of cast iron, each eight inches high and three inches and a-half in diameter, with the same number of amalgamated zinc cylinders of about two inches diameter. Although the electro-motive force of an iron zinc pair is only about one-half that of a Daniell's cell, Mr. Sturgeon's cast iron battery may be advantageously employed in many cases, owing to its great simplicity and cheapness.

In an experimental investigation of the "Magnetic Characters of Simple Metals, Metallic Alloys, and Salts," published in the seventh volume of the Transactions of this Society, Mr. Sturgeon showed that several metallic alloys became endued with magnetic properties, although their constituents separately showed no such action. On the other hand, nickel and iron became almost entirely inert to magnetic action when combined with other metals. An alloy of iron

and zinc, in the proportion of 1 to 7, was found to be quite destitute of magnetic action.

In 1839 he published a memoir "On Marine Lightning Conductors," comprising an examination of the effects of lightning on shipping, with remarks on Mr. Harris's plan; and the description of a new system of conductors. Mr. (now Sir W. Snow) Harris, following Mr. Henley, placed a strip of copper into a groove let into each mast. An electrical discharge striking the top of the mast was thus to be conducted by the copper strip through the keel into the water. In the system recommended by Mr. Sturgeon, copper rods or metallic ropes are placed aft the shrouds of each mast. The upper extremities of these conductors are attached to the fore, main, and mizen tops, as distant from the masts as circumstances will allow. The lower ends are connected with the chains belonging to the aft shrouds of each mast, and continued by broad and stout straps of copper to the sheathing of the ship. The top and top-gallant masts are protected in a similar manner, their conductors being connected with those of the lower masts. If, therefore, lightning strike the top-gallant, it will descend in two streams on either side of the mast, and then over the sides into the sea. Lastly, in order to distribute the discharge as completely as possible, and thus still further to lessen its danger, he proposed to unite the conductors of the fore, main, and mizen, by means of copper rods or metallic ropes in the position of stays. Little attention seems to have been paid to the above system, which, nevertheless, secures many highly important advantages, of which the following may be enumerated. 1st. The conductors being for the most part entirely removed from any combustible material, and combined so as to divide amongst them any stroke of lightning, the danger of fire in consequence of lateral discharge is less to be apprehended. 2nd. The electric fluid is carried over the side of the vessel, which

obviates the great risk which might arise from lateral explosion near the combustible or explosive materials of a cargo, and particularly should any accident have occasioned any interruption of the complete continuity of a conductor passing through the hull. 3rd. The magnetizing effects on chronometers are diminished. And 4th. The labour and expense of the fitting a ship with conductors are considerably lessened. I have said that Mr. Sturgeon's system, however excellent, did not receive much attention from the existing authorities. In addition to this disappointment, he found himself involved in a dispute with Mr. Harris, who had been largely successful in introducing Mr. Henley's plan into the navy. The advancement of science is seldom facilitated by warm controversy, and in this instance it must be allowed that Mr. Sturgeon suffered himself to be betrayed into stronger language than was suitable to the occasion. But with respect to the scientific portion of the argument, I cannot but be of opinion that he had a decided advantage over his opponent. The chief subject of dispute was whether a vertical conductor communicating by its lower end with the earth will, when struck by lightning, produce strong electric disturbance to vicinal conducting bodies not in metallic connexion with the earth. Mr. Harris insists that the spark which is observed to pass between an insulated discharging rod and an insulated conductor very near it, is owing to an excess of electricity on one coating of the leyden jar, as compared with that on the other; so that complete neutralization not taking place, the discharging rod receives a charge which it is able to communicate to neighbouring bodies. This hypothesis is, however, entirely at variance with Dr. Priestley's investigation of the phenomenon, as well as the experiments of Weekes, and many others. But there can be no doubt that even admitting Mr. Harris's hypothesis, the lateral explosion must exist pre-eminently in a thunderstorm, the circumstances of which render it almost impossible to conceive that the total electricity of a cloud is just equal in amount to

the electricity induced by it on the earth's surface. But even were the lateral explosion disproved, it could not be denied that any electrical current must at its rise and fall tend to produce currents of induction, according to the laws discovered by Faraday and Henry. Such induced currents must, in the case of a lightning stroke, be excessive, and only estimable by direct experiments on conductors in the neighbourhood of one struck by lightning. It must also be remarked that any discharge will, according to the established laws of conduction of electric currents, distribute itself among all bodies in contact with the lightning conductor, in proportion to their conducting powers. The portion thus conveyed might indeed be numerically insignificant as compared with the main stream, but yet sufficient to create sparks, at disruptions of such weak conductors, capable of igniting combustible materials. For the above reasons, whatever good fortune may have hitherto attended the conductors at present used in the navy, I cannot but prefer Mr. Sturgeon's system, which removes the conductors as far as possible from contact with any portion of the ship or its cargo.

Atmospheric electricity was a subject to which Mr. Sturgeon devoted a great deal of attention, from the commencement of his scientific career. He was in the habit of raising a kite, the string of which, insulated at the lower end, had a fine wire laid in it. In all seasons and weathers, on high and low ground, in every hour of both day and night, did he pursue this interesting but somewhat hazardous* course of investigation. By the result of more than five hundred kite observations he confirmed the important fact, first discovered by Kinnersley and Beccaria, that the atmosphere in serene weather is uniformly positive with regard to the earth. He also proved that the higher we ascend the more positive does it become;

* See his "Caution to Experimenters with the Electrical Kite," which he published in consequence of having been nearly killed by a discharge from only one hundred yards of wired string, though no thunder was heard.

so that if the strata in which the kites are immersed are at altitudes corresponding to the series 1, 2, 3, 4, 5, their relative charges of positive electricity would be conveniently represented by those numbers. He also revived copper and silver from their solutions, and decomposed water by atmospheric electricity. I cannot forbear describing, in his own words, one of the many extraordinary electrical displays, of which Mr. Sturgeon was a witness. On June 14th, 1834, he writes: "The rain fell so heavily that it was with some difficulty I got the kite afloat; and when up, its greatest altitude did not exceed fifty yards. The silken cord also, which had been intended for the insulator, soon became so completely wet that it was no insulator at all. Notwithstanding all these impediments, I was much gratified with the display of the electric matter issuing from the end of the string to a wire, one end of which was laid on the ground and the other attached to the silk, at about four inches distance from the reel of the kite string. An uninterrupted play of the fluid was seen over the four inches of wet silken cord, not in sparks, but in a bundle of quivering purple ramifications, producing a noise similar to that produced by springing a watchman's rattle. The display was beautiful beyond description. The reel was occasionally enveloped in a blaze of purple arborized electrical fire, whose numberless branches ramified over the silken cord, and through the air to the blades of grass, which also became luminous on their points and edges, over a surface of some yards in circumference. We also saw a complete globe of fire pass over the silken cord between the wire and reel of the kite string. It was exceedingly brilliant, and about the size of a musket ball."

Having on another occasion elevated an electrical kite during a thunderstorm, he noticed that a shower of sparks was discharged from the string at the moment of each flash of lightning, a phenomenon which he accounted for by supposing that every flash of lightning disturbs all the electric

fluid around it, and thus produces an *electrical wave* which, in some instances, reaches to great distances.

Mr. Sturgeon noticed that thunderstorms are the most violent when they occur over wet ground or water, owing to the facility with which such a surface can be charged by the inductive influence of electrified clouds; and also that electrical storms generally travel in the direction of rivers or along narrow tracts of wet ground. In his description of the thunderstorm which occurred near Manchester on the 16th July, 1850, he made an observation which was probably more remarkable than any that had been previously made in atmospheric electricity. This consisted in the determination of the *time* of a lightning discharge by means of the vibrations of a pendulum; the *streams* of electric fluid being visible in some cases as long as a second and a-half. Mr. Clare and others, including myself, who witnessed the same phenomenon from different points of view, coincided in remarking that these streams of electric fluid took up a very sensible time, and that the motion of electricity along them was apparent. This extraordinary and magnificent spectacle seems to have been owing to the great elevation at which the lightning was playing, where the rarefaction of the air would probably cause it to assume some of the characteristics of the aurora borealis.

Mr. Sturgeon does not appear to have embraced any theory of the cause of thunderstorms. I may, however, remark, that after the discovery by Faraday that the enormous evolution of electricity in Armstrong's hydro-electrical apparatus is owing to the friction of particles of water and air, it is probable that the electricity of the thunder-cloud arises from the friction of the particles of water or ice of which it is composed, or which are falling from it, against the cold non-conducting air which exists at a high altitude.

The aurora borealis was a phenomenon in which Mr. Sturgeon took a great deal of interest. His observations of this

meteor extended over a period of more than twenty years. Its cause he attributes to a sudden change of temperature in the upper regions of the atmosphere, giving rise to a corresponding disturbance of the electric fluid. The attenuated air is, he conceives, illuminated by these extensive movements of electricity, which at the same time agitate the magnetic needle in conformity with the laws of electro-magnetic action. I think it must be allowed that a more intimate connexion subsists between the magnetism of the earth and the aurora than he seems disposed to admit, although few will, probably, embrace the very artificial hypothesis of Dalton. Mr. Sturgeon shows an excellent example of the spirit with which all mere hypothesis ought to be enunciated, in the concluding sentence of his "Observations on the Aurora Borealis," in which he says with much modesty, "It is possible, however, that the theoretical views which I have here advanced may be open to objections that I do not myself perceive, and may require the corrections of a more diligent observer, and a sounder reasoner on the facts observed."

The last contribution I shall notice is one on a subject of great scientific interest and practical importance, "the electro-culture of farm crops." Many philosophers of eminence had made the observation that plants grow more vigorously when they are electrified or galvanized; but the experiments of Dr. Forster, in 1845, appear to have been the earliest in which the atmosphere was employed as the electrifying agent. He suspended a wire in the air communicating metallically with a wire buried three inches deep and surrounding the plot experimented on. He found the yield of the ground, so electrified, to be considerably greater than that of the rest of the field. The experiments of Dr. Forster, variously modified, were repeated by Mr. Sturgeon with results sufficiently successful to encourage further investigation. I do not know whether any further trials have been made, but when we reflect upon the enormous quantity of electricity

almost constantly descending from the air through the leaves and roots of plants, it is impossible not to allow that great influence on vegetation is exercised by it, and it must be reasonable to infer that such influence may be increased or modified by collecting and distributing it by artificial means.

Having completed a rapid survey of Mr. Sturgeon's principal scientific researches and discoveries, it will be proper now to notice his career in the several capacities of editor, lecturer, and professor. Soon after leaving the army he was appointed Lecturer on Experimental Philosophy to the Hon. East India Company's Military Seminary at Addiscombe, and he continued to hold that position, with credit to himself, until, in the year 1838, he accepted the office of Superintendent of the Royal Victoria Gallery of Practical Science, an institution which was founded in this city by a few public spirited gentlemen, in the hope that by means of popular scientific discourses, illustrated by experiments with apparatus of a size and completeness unattainable by a private lecturer, interest would be excited and scientific education largely promoted. This expectation was, however, unhappily disappointed. The indifference to pursuits of an elevated character, which too frequently marks wealthy trading communities, destroyed this, as it has many other useful institutions. Undismayed by this failure, Mr. Sturgeon made strenuous efforts to establish another institution of a similar character, but which failed from the same cause which ruined its predecessor. After this he had no further connexion with any other educational establishment, and relied for support on the precarious emoluments arising from the courses of lectures he delivered in various parts of the country.

As a lecturer he was distinguished by his power of impressing the truths of science clearly and accurately on the minds of his auditory, and especially by the uniform success of his experimental illustrations. The following quotation, from one of his unpublished lectures, illustrating at once his

style of composition and his comprehensive view of the electrical agent, will be read with interest.—" Thus, ladies and gentlemen, I have, as far as this short course of lectures would admit, endeavoured to illustrate the principles of magnetism, and their connexion with electricity. I have shown you by a series of selected experiments, that there is not only a reciprocal action exercised by those mysterious powers upon each other; but, by *their* agency, some of the most wonderful and important phenomena of nature are produced.

" The electric fluid is so universally diffused throughout every part of nature's productions, that every particle of created matter, both animate and inanimate, which has hitherto been contemplated by the philosopher, is full of this surprizingly animated elemental fire.

" In regions far above the surface of the earth, where the air is much attenuated and so far thinned, near to the utmost verge of the atmosphere, as to become a conducting medium, the *electric element* plays its quivering streamers and sparkling corruscations in the beautiful aurora of the north. Sometimes this rare—this fascinating phenomenon is exhibited in a steady glowing arch of light; whilst at others, it expands its dancing network in transient display over the whole concave of the visible heavens.

" At altitudes less elevated than those which form the grand theatre for the display of the aurora borealis, the electric discharges become more compact, and shoot slanting downwards, on bright serene evenings, those beautiful gleaming orbs of meteoric light, which, from ancient custom, are still called falling stars.

" Still less elevated in the atmosphere the big black clouds swell with the *electrical element*, until bursting from its aerial walls it discharges itself into space, in all those grand, magnificent, and splendid forms of lightning, with their tremendous peals of thunder, so frequently displayed in most countries during the transient rage of a majestic summer's storm.

"Descending to the earth we trace its circumfluent streams polarizing this vast ball of matter, on which we are destined to live and perform all our actions, and insinuating its resistless influence in all the silent, mysterious, attractions of the *magnet*.

"Trace it to the laboratory of the chemist, and we find it the most active and vigorous agent in accomplishing all those astonishing changes which give new forms, and new qualities, to passive, obedient matter.

"Besides all these important operations of nature, accomplished by the agency of electricity, it is capable of restoring the dormant muscular and nervous powers of man, which have been prostrated by accident or disease; and of giving new life, and new vigor, to parts which have bid defiance to every other mode of medical treatment.

"In plants also, as well as in animals, it is said to facilitate their growth, and to give health, vigor, and beauty to their general appearance.

"Indeed, so *universally* does the electric fluid appear to be employed in *most*, if not *all*, the grand processes of nature, that there is not, perhaps, a plant that grows, nor a limb that moves, but is, in some measure, influenced by its powers.

"Nay, it is, perhaps, this astonishing, this most gigantic of physical agents, which is employed by the GREAT CREATOR to spin the earth and planets on their axes, to sweep them through the heavens in their regular periods of revolutions, and to keep in uniform motion all those massy orbs of matter which compose the countless systems of the universe.

"Brief and imperfect as is the outline which I have thus portrayed of *one individual* branch of science, perhaps we may venture to ask, who is there, then, who knows the advantages, the beauties, nay, the pleasures of scientific knowledge, who would think his time misspent, or his labours useless, in the accomplishment of so noble an acquisition?

"It is by the cultivation of the mind that one man be-

comes superior to, and has an absolute advantage over, another; and, by the study of science, the minutest process, and the most stupendous operation of nature, become alike delightful and familiar. The mind is thus led to explore the beautiful, the harmonious, the wondrous *works* of creation, and to admire and adore Creation's GOD."

In the year 1836 Mr. Sturgeon established a new scientific periodical, entitled the "Annals of Electricity, Magnetism, and Chemistry, and Guardian of Experimental Science," which he in the first instance published quarterly, but afterwards monthly. He conducted it with great ability, industry, and perseverance through ten octavo volumes. This work became the medium of the communication of much valuable information; and we are told that it gave rise to a similar publication, edited by Professor De la Rive, of Geneva. Subscribers to it were not sufficiently numerous to enable Mr. Sturgeon to continue it; but, nevertheless, after an interval of six months he made another attempt, which resulted in the appearance of the "Annals of Philosophical Discovery," which, however, he gave up after publishing six monthly numbers. Few, unhappily, care to read what it takes the slightest effort to understand; hence, with certain special exceptions, the sale of a publication may almost be said to be inversely proportional to the cultivation that may be expected from its perusal. Thus an inferior periodical literature is most generally read, while so little regard is paid to the phenomena of nature, or the theories of the philosopher, that a scientific magazine is nearly always a bad speculation, in a pecuniary point of view.

Mr. Sturgeon published several useful elementary works, of which the principal are:—"Lectures on Electricity, delivered in the Royal Victoria Gallery in 1841-42;" "A Course of Twelve Elementary Lectures on Galvanism;" and "A Familiar Explication of the Theory and Practice of Electro-gilding and Electro-silvering." He also prepared a transla-

tion of Jacobi's "Whole Galvanoplastic Art," and a new edition of Barlow's "Treatise on Magnetism." His last work, which was completed only a few weeks before his death, was a compilation, in one large quarto volume, of the whole of his published scientific papers, systematically arranged, preceded by an able historical sketch of electro-magnetism, from its commencement until the year 1823.

That a man, whose whole life was spent in hard labour, both mental and bodily, and in the anxieties and deprivations incident to slender pecuniary resources, should have attained to the age of even 67 years, is somewhat remarkable, and shows, what other similar cases testify, how intimate a connexion subsists between the strength of the mind and the stamina of the constitution. Or, rather, does it not teach that the brain being kept under steady discipline, and in constant active employment, reacts on the body so as to produce a corresponding vigor in the functions by which life is sustained? Mr. Sturgeon continued, with little intermission, in a course of assiduous industry until ten days before his death, which took place at Prestwich, on Sunday, December 8th, 1851.

In considering the moral and intellectual qualities of this eminent man, it is needful to recollect that from his birth to 1820, an interval of 37 years elapsed, during which the circumstances of his position precluded almost entirely the cultivation of the higher faculties of the mind. Thus, to him, were wanting all those advantages which belong so eminently to early study; such as the stimulus of first ideas; freshness and pliability of imagination; the vigour and hopefulness of a mind unsoured with disappointment, and unclogged with the infirmities which begin to settle upon it when the meridian of life is scarcely attained. That Mr. Sturgeon succeeded at all with such a fundamental disadvantage, can only be attributed to the amazing elasticity and sanguineness of his temperament. This quality, so essential

to his position, and without which he would probably never have attained even mediocrity, must, nevertheless, be reckoned as the source of many of his embarrassments. On the one hand it made him look beyond and aim further than the mere acquisition of personal comfort; on the other hand it led him to embark in projects which entailed upon him trouble and loss. So true it is, that even the best qualities of the mind are to a certain extent, so to speak, double handed, and affect a man's happiness injuriously or beneficially according to the character of the individual, and the circumstances in which he is placed.

To see a man labouring hard for the good of society and the advancement of science without the possession of suitable means, is a spectacle which calls forth the liveliest sympathies of our nature. However noble the motives of such exertion, it is certain that a man is frequently placed in circumstances which render it a duty and virtue to deny himself the gratification of intellectual pursuits. If a family is dependent upon his labour for their daily bread,—if the claims of creditors are to be met,—then it may be his bounden duty to pursue the ordinary employment of society rather than his favourite researches, however important, if they are not accompanied by a steady and adequate remuneration. Frequently, however, it is difficult, or even impossible, for a man disappointed in his hopes of realizing an income by intellectual labour, to turn his attention successfully to ordinary business, and thus paralysed in all his efforts he becomes involved in crushing and hopeless penury. The sum which would be necessary to succour the needy man of science, and so to enable him to continue his researches, would appear trifling indeed if regard were had to the important objects to be realized. But he appeals not to his country as a pauper. He asks it to discharge the debt it owes for labours which have contributed to the common weal, a debt which cannot honourably be left unpaid.

In the case of Mr. Sturgeon, Government was induced, through the untiring exertions of my friend, E. W. Binney, F.R.S., &c. (to whom science owes so much for his kind assistance to its humble cultivators), to award a sum of £200., and afterwards a pension of £50., which, on Mr. Sturgeon's death, a year afterwards, was continued to his widow. These sums, along with the private efforts of his friends, alone saved him from being reduced to a state of destitution, which had it occurred would have reflected deep discredit upon the community.

Mr. Sturgeon was of a tall and well built frame of body; his forehead was high, and his features were strongly marked. His address was animated; and his conversation, as it generally is when the mind is stored with knowledge, pleasing and instructive. His style of writing was at once vigorous, lucid, and graceful. In friendship he was warm and steady; in domestic life affectionate and exemplary. He had a noble mind and a generous heart. In politics he was a conservative; in religion a member of the Church of England. He was a close and sagacious reasoner, and an unsparing exposer of error. He detested quackery and false pretension, sought diligently for truth, and loved it for its own sake. A diligent accumulator and observer of facts; eager in the pursuit of information, of whatever kind, and in communicating his stores to others; under more fortunate circumstances he would, probably, have left a name unsurpassed in the scientific history of his time; as it is, he will always be remembered as a distinguished cultivator of natural knowledge.

VI.—*On the Solubility of Sulphate of Baryta in Acid Solutions.*

By F. Crace Calvert, F.C.S., M.R.A. Turin, &c.

[*Read December 2nd*, 1856.]

There are few substances more frequently met with and which require to be determined with greater precaution than sulphur and its compounds, it is therefore of the highest importance to possess exact means of knowing their proportions.

Up to the present time it has been admitted that sulphate of baryta was so insoluble that it was sufficient to add a soluble salt of baryta to a solution containing a sulphate, with an excess of acid, either nitric or hydrochloric (so as to avoid the precipitation of other acids by the baryta), in order that the sulphuric acid existing in a liquor should be exactly determined. My researches have convinced me that sulphate of baryta *is* soluble in acids. I have succeeded, as will be seen further on, in dissolving two grains of sulphate of baryta in 1,000 grains of nitric acid of a specific gravity of 1.167; whilst it requires 140,000 grains of water to dissolve one grain of sulphate of baryta; and what is not less interesting is, that very weak nitric acid influences the solubility of sulphate of baryta; it is therefore essential not to acidify liquors with nitric acid.

This fact seemed to me so important for analytical chemistry, that I made a great number of researches in order to

know all the circumstances which influence the solubility of sulphate of baryta; and, although I have made several hundred experiments, I shall only give here the most striking, and those which tend to demonstrate the influence exercised by masses of matter, upon their chemical affinity. I hope that these last facts will offer the more interest, as we only possess few researches demonstrating, that Berthollet expressed a correct opinion, when he asserted that the quantity of matter present exercised an influence upon chemical affinity. However, since I have commenced these researches, in 1851, several works supporting these views have appeared, those of Messrs. Bunsen, Gladstone, Henry Deville, &c.

The first idea of these researches was suggested to me by an inquiry which I had undertaken to determine the quantity of sulphur in cast and malleable irons. As the quantity of sulphur to be determined only varied between the limits of 0.1 and 0.3, it was indispensable that I should take the greatest precautions; but, notwithstanding, I still found it impossible, in making two comparative analyses of the same iron, to obtain two sulphates of baryta having corresponding weights. Desiring to know the cause of error I made several experiments, and discovered, to my great surprise, that sulphate of baryta was freely soluble in acids, and then the error in my analyses was explained. Some quantitative analyses of water, which I had to make at the same time, led me to the same result; for having followed the ordinary process which consists in adding, in the case of a water which contains carbonates and sulphates, nitric acid to the residue left by the evaporation of a known quantity of water, I determined the quantity of sulphuric acid, and was again struck by the divergence of my results. If I had contented myself by taking the mean of the results obtained, I should have been obliged, in calculating the relation of the bases to the acids, to admit, as some chemists have done, that there were silicates

of magnesia and lime in the water, whilst by avoiding the solution of the sulphate of baryta, I arrived at the conclusion that the bases existed as sulphates and that the silica was free, or combined in small quantity with potash or soda.

Before giving the results of my experiments, which demonstrate the solubility of sulphate of baryta in acid solutions of different degrees of concentration, and the influence exercised by multiple volumes of nitric acid of specific gravity 1.167 on the same solubility, I shall give the method of operating which I followed.

Having placed in ten jars, of the same size, known quantities of perfectly pure sulphate of potash, I added to them a given bulk of nitric acid of sp. gr. 1.305, and a quantity of distilled water, such as when added to that which was employed to dissolve the nitrate of baryta, made a known total, which was in each successive jar a multiple of the quantity of water put in the preceding jar. When, instead of employing acids decreasing in strength, I wished to study the influence of increased volumes of the same acid, that used was of the sp. gr. 1.167 at 60°, or equal to 27.27 of anhydrous nitric acid and 72.73 of water per cent. This strength of acid was employed in preference to any other, because it was that which gave the *maximum* of solubility to the sulphate of baryta. I also took the precaution to use new jars, in order to avoid scratches, which influence the solubility of the sulphate of baryta; and equally avoided agitating the intimate mixture of the two salts, in order to be always, as far as possible, placed in the same circumstances, so that the results might be comparative.

I will give, first, several series of results which demonstrate the solubility of the sulphate of baryta; and afterwards will examine the facts which have special relation to chemical affinity.

SOLUBILITY OF SULPHATE OF BARYTA IN MULTIPLE VOLUMES OF NITRIC ACID.

The first series of experiments was made by placing in each jar a quantity of sulphate of potash, capable of producing 1.00 gr. of sulphate of baryta, and adding successively in each jar, multiple volumes of nitric acid having a sp. gr. of 1.167 at 60°, and on the sulphate being dissolved, pouring into it an equivalent quantity of nitrate of baryta, previously dissolved in a known volume of water.

TABLE No. 1.

Order of Jars.	Number of Divisions of the Alkali-meter.	Corresponding Weight of Acid. Sp. gr. 1.167	Quantity of Sulphate of Potash.	Quantity of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	40	466.8	0.735 gr.	1.121	0 999	Only in No. 1 did a cloud appear after 12 hours, and even after 24 hours the precipitate was very faint. No precipitate in the other nine jars.
2	80	933.6	......	......	......	
3	120	1400.4	......	......	......	
4	160	1867.2	......	......	......	
5	200	2334.0	......	......	......	
6	240	2800.8	......	......	......	
7	280	3267.6	......	......	......	
8	320	3734.4	......	......	......	
9	360	4201.2	......	......	......	
10	400	4668.0	......	......	......	

On examining the above results, it will be observed that one grain of sulphate of baryta was almost entirely dissolved in 40 volumes of acid, or in 466.8 grs. of nitric acid, and entirely so in 933.6 grs. of the same acid, and that no precipitate was formed in the other eight jars, even after standing forty-eight hours. There was therefore more than 1 gr. of sulphate of baryta dissolved in 1000 grs. of acid. Desiring to obtain a precipitate in all the ten jars, I employed 4½ times the above quantity of nitrate of baryta and sulphate of potash, and obtained the following results.

TABLE No. 2.

Order of Jars.	Number of Divisions of the Alkalimeter.	Corresponding Weight of Acid. Sp. gr. 1.167	Quantity of Sulphate of Potash.	Quantity of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	40	466.8	3.34	5.00	4.46	instantly.
2	80	933.6	......	......	......	20 minutes.
3	120	1400.4	.....	.....	......	2 hours.
4	160	1867.2	......	......	......	8½ hours.
5	200	2334.0	......	......	......	24 hours.
6	240	2800.8	......	......	......	no precipitate.
7	280	3267.6	......	......	......	ditto
8	320	3734.4	......	......	......	ditto
9	360	4201.2	....	......	......	ditto
10	400	4668.0	... ..	......	......	ditto

I was much surprised to find, that even after standing twenty-four hours, no precipitates were formed in jars Nos. 6, 7, 8, 9, and 10, although the quantity of salt employed was capable of giving rise to 4.46 of sulphate of baryta.

These results are also interesting in another point of view, viz., if we calculate the quantity of sulphate of baryta dissolved by 1000 grs. of acid in jar No. 6, the quantity found is 1.593, or in other terms, there is .592 more salt dissolved per 1000 grs. of acid, than in the jar No. 2 of No. 1 Table; this difference of solubility can only be owing to the influence exercised by a greater quantity of sulphate of potash and nitrate of baryta being employed, since everything else was equal. In pursuing the reading of this paper it will be seen that the influence exerted by quantity on mass is clearly confirmed. Having remarked that in the jars, Nos. 1, 2, 3, 4, and 5, of the above series, the precipitates diminished rapidly in quantity, I collected them, and washed them until the liquor gave no precipitate, either with sulphate of potash or nitrate of baryta; but these washings are very tedious, the nitrate of baryta or the sulphate in excess adhering to the sulphate of baryta with extraordinary tenacity; and this difficulty, as I will show further on, increases still more when the recom-

mendation given by some chemists is followed, viz., to add a great excess of nitrate of baryta to the liquor containing the sulphates.

TABLE No. 3.

Order of Jars.	Number of Divisions of the Alkalimeter.	Corresponding Weight of Acid. Sp. gr. 1.167.	Quantity of Sulphate of Baryta Precipitated.	Quantity of Sulphate of Baryta Dissolved.	Quantity of Sulphate of Baryta dissolved in 1000 grains.
1	40	466.8	4.44	0.02	0.043
2	80	933.6	3.17	1.29	1.382
3	120	1400.4	2.02	2.34	1.670
4	160	1867.2	0.80	3 66	1.957

These figures show not only the rapid increase of solubility of sulphate of baryta in multiple volumes of nitric acid sp. gr. 1.167, but also that the formation of a precipitate does not prevent the great solubility of this salt.

Thus, in jar No. 2, there is 1.382 dissolved in 1000 grs.
„ No. 3, „ 1.670 „ „
„ No. 4, „ 1.957 „ „

Therefore in each successive jar there is 0.3 gr. more of sulphate of baryta dissolved for the same volume of acid.

In the hope of producing a precipitate in all the jars of the series, I employed a greater quantity of salts for the same proportions of acid as above, and the results are given in the following table.

TABLE No. 4.

Order of Jars.	Number of Divisions of the Alkalimeter.	Corresponding Weight of Acid. Sp. gr 1.167	Quantity of Sulphate of Potash.	Quantity of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	40	466.8	5.12	8.00	7.13	instantly.
2	80	933.6	......	......	......	2 minutes.
3	120	1400.4	......	......	......	14 minutes.
4	160	1867.2	......	......	......	1 hour.
5	200	2334.0	......	......	......	1 hour 15 minutes.
6	240	2800.8	......	......	......	4 hours.
7	280	3267.6	......	......	......	8 hours.
8	320	3734.4	......	......	......	24 hours.
9	360	4201.2	......	......	......	none.
10	400	4668.0	......	......	......	none.

Although the quantity of salt employed in this case was capable of producing 7.13 grs. of sulphate of baryta, still, after twenty-four hours there was no precipitate produced in jars No. 9 and 10, and in order to obtain a precipitate in all the jars composing a series, I was obliged to employ quantities of salts of baryta and potash, capable of producing 10.0 grs. of sulphate of baryta.

In order to confirm the results given in Table No. 3, the precipitates formed in the above series were collected, and their weight determined.

TABLE No. 5.

Order of Jars.	Number of Divisions of the Alkalimeter.	Corresponding Weight of Acid. Sp. gr. 1.167.	Quantity of Sulphate of Baryta Precipitated instead of 7.13.	Quantity of Sulphate of Baryta Dissolved.	Quantity of Sulphate of Baryta dissolved in 10.0 grains
1	40	466.8	6.86	0.27	0.591
2	80	933.6	5.63	1.50	1.615
3	120	1400.4	4.66	2.47	1.767
4	160	1867.2	3.22	3.91	2.099
5	200	2334.0	2.33	4.80	2.059
6	240	2800.8	1.10	6.03	2.155
7	280	3267.6	0.14	6.99	2.141

These results prove that 1000 grs. of nitric acid sp. gr. 1.167, are able to dissolve about 2 grs. of sulphate of baryta.

ON THE INFLUENCE EXERCISED BY AN EXCESS OF PRECIPITANT.

As it is generally admitted that an excess of precipitant increases the chances of obtaining the total precipitation of the sulphate in solution, I thought it useful to make a series of experiments to ascertain if these views were correct, and it will be seen, by comparing the results given below with those of Table No. 2, in which equivalent quantities were used, that such is not the case, since the results in the two tables are identical.

TABLE No. 6.

Order of Jars.	Number of Divisions of the Alkali-meter.	Corresponding Weight of Acid. Sp. gr. 1.167	Quantity of Sulphate of Potash.	Quantity of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	40	466.8	3.34	10.00	4.46	instantly.
2	80	933.6	......	......	......	5 minutes.
3	120	1400.4	......	......	......	37 minutes.
4	160	1867.2	....	......	......	2 hours 30 minutes.
5	200	2334.0	......	......	......	5 hours.
6	240	2800.8	......	......	......	24 hrs. distinct cloud
7	280	3267.6	......	......	......	no precipitate.
8	320	3734.4	......	......	......	ditto
9	360	4201.2	......	......	......	ditto
10	400	4668.0	......	......	......	ditto

But this method of operating, namely, employing an excess of precipitating agent, offers, as I have already said, great inconveniences in consequence of an excess of precipitant adhering to the sulphate of baryta, which necessitates numberless washings. To avoid prolonging this paper, I shall state that the same difficulties present themselves when two equivalents of sulphate of potash are employed for one equivalent of nitrate of barytes.

INFLUENCE OF BOILING, UPON THE FORMATION OF SULPHATE OF BARYTA.

TABLE No. 7.

Order of Jars.	Number of Divisions of the Alkali-meter.	Corresponding Weight of Acid. Sp. gr. 1.167	Quantity of Sulphate of Potash.	Quantity of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	40	466.8	3.34	5.00	4.46	instantly.
2	80	933.6	......	......	......	3 minutes.
3	120	1400.4	......	......	......	8 hours.
4	160	1867.2	......	......	......	24 hours.
5	200	2334.0	......	......	......	no precipitate.
6	240	2800.8	......	......	......	ditto
7	280	3267.6	......	......	......	ditto
8	320	3734.4	......	......	......	ditto
9	360	4201.2	......	......	......	ditto
10	400	4668.0	......	......	......	ditto

On comparing the results contained in this table with those in Table No. 2, it is observed that the formation of sulphate of baryta is not facilitated by boiling. This fact was often confirmed during the course of my experiments, by taking the clear solutions which remained when the precipitate of sulphate of baryta had subsided and bringing them to ebullition, without in any case obtaining a precipitate.

EMPLOYMENT OF SULPHURIC ACID INSTEAD OF SULPHATE OF POTASH.

I thought it right to make the following series of experiments, in order to ascertain that the non-formation of sulphate of baryta, in a liquor rendered acid by nitric acid, was not owing to a combination being formed between the sulphate of potash and the nitric acid; to attain this object, I poured into each multiple volume of nitric acid a quantity of sulphuric acid, equivalent to that contained in 3.34 grs. of sulphate of potash, and the results obtained were identical with those observed in Table No. 2; consequently no combination was formed between the nitric acid and the sulphate of potash.

TABLE No. 8.

No. of Jar.	Number of Divisions of the Alkalimeter.	Corresponding Weight of Acid. Sp. gr. 1.167	Sulphuric Acid, equal to 1.533, capable of producing 4.46 $BaOSO_3$	Weight of Nitrate of Baryta.	Time when a precipitate appeared.	Weight of Nitrate of Baryta.	Time when a precipitate appeared.
1	40	466.8	1.533	5.00	instantly.	10.00	instantly.
2	80	933.6	......	......	13 minutes.	......	6 minutes.
3	120	1400.4	......	......	2 hrs. 30 min.	......	30 minutes.
4	160	1867.2	......	......	7 hours.	......	1 h. 15 min.
5	200	2334.0	.. ...	......	24 hours.	......	5 h. 30 min.
6	240	2800.8	......	......	no precipitate	......	7 h. 30 min.
7	280	3267.6	......	......	ditto	......	24 hours.
8	320	3734 4	......	......	ditto	......	no precipitate.
9	360	4201.2	......	......	ditto	......	ditto
10	400	4668.0	.... .	......	ditto	......	ditto

I also made several series of experiments, dissolving first the nitrate of baryta in the nitric acid, and then pouring into it the sulphate of potash; but this mode of operating did not

produce any alteration in the results obtained. During this series of experiments, I determined the solubility of nitrate of baryta in nitric acid sp. gr. 1.167 at a temperature of 60°.

1 grain	of nitrate	of baryta	requires	250.2
2.5	,,	,,	,,	446.8
5.0	,,	,,	,,	933.8
6.5	,,	,,	,,	1334
8.0	,,	,,	,,	2800
10.0	,,	,,	,,	3267

ON THE INFLUENCE EXERCISED BY DIFFERENT DEGREES OF CONCENTRATION OF NITRIC ACID ON THE FORMATION OF SULPHATE OF BARYTA.

In the following series of experiments I operated in an entirely different manner; the same quantity of nitric acid, sp. gr. 1.305, was put into each jar, and to each was added multiple volumes of water; therefore, each solution decreased in density; and after having dissolved the quantity of sulphate of potash which I intended to employ, I poured in the solution of nitrate of baryta.

On comparing the experiments contained in the following table, it will be seen that I have obtained results very different from the preceding, for instead of 4.46 of sulphate of baryta remaining in solution in the last five jars, as was the case when I employed the same quantities of salt in multiple volumes of the same acid, precipitates were found in all the jars, and that in the space of three minutes; therefore, multiple volumes of acid, decreasing in strength, do not exert the same influence as the increase of volume of the same acid. This table also presents a fact important for chemical analysis, viz., that the solubility of sulphate of baryta is affected even by the weakest acid, since 2200 grs. of nitric acid, sp. gr. 1.032, are capable of dissolving 0.08 of sulphate of baryta; this solubility, though slight, is still five times greater than it would be in distilled water.

TABLE No. 9.

Order of Jars.	Number of Divisions of the Alkalimeter of Nitric Acid Sp. gr. 1.305.	Number of Divisions of the Alkalimeter of Water added to the Acid.	Sp. gr. of the Acid.	Sulph. of Potash dissolved in the Acid.	Nitrate of Baryta.	Time when a precipitate appeared.	Quantity of Sulphate of Baryta precipitated instead of 4.46.	Quantity of Sulphate of Baryta dissolved.
1	20	20	1.167	3.34	5.00	3 minutes	4.28	Average
2	20	40	1.120	......	......	......	4.34	quantity
3	20	60	1.085	......	......	......		dissolved
4	20	80	1.067	......	......	......		equal to
5	20	100	1.057	......	......	......	4.35	0.10.
6	20	120	1.050	......	......	......	4.35	
7	20	140	1.044	......	......	......	4.36	
8	20	160	1.039	......	......	......		
9	20	180	1.035	......	......	......		
10	20	200	1.032	......	......	......	4.38	

The series of experiments which follows, was made with the view of knowing what would be the influence of a less quantity of nitrate of baryta for the same quantity of sulphate of potash, and it will be seen, in perusing the table, that instead of obtaining a precipitate in all the jars in three minutes, the time required was from 2 hours 30 minutes to 24 hours, although, with the exception of the quantity of nitrate of baryta, everything was equal.

TABLE No. 10.

Order of Jars.	Number of Divisions of the Alkalimeter of Nitric Acid Sp. gr 1.305.	Number of Divisions of the Alkalimeter of Water added to the Acid.	Sp. gr. of the Acid.	Sulphate of Potash dissolved in the Acid.	Nitrate of Baryta poured in.	Time required for a Precipitate to appear.
1	20	20	1.167	3.34	0.25	2 hours 30 minutes.
2	20	40	1.120	......	......	2 hours 45 minutes.
3	20	60	1.085	......	......	2 hours 45 minutes.
4	20	80	1.067	......	......	6 hours.
5	20	100	1.057	......	......	6 hours.
6	20	120	1.050	......	......	24 hours.
7	20	140	1.044	......	......	24 hours.
8	20	160	1.039	......	......	24 hours.
9	20	180	1.035	......	......	24 hours.
10	20	200	1 032	......	......	24 hours.

I thought that it would be interesting to know what would be the *minimum* quantity of nitrate of baryta necessary, in

order that no precipitate should be formed in any of the jars of the series, when 3.34 of sulphate of potash was in solution, and the following table gives the results obtained.

TABLE No. 11.

Order of Jars.	Number of Divisions of the Alkalimeter of Nitric Acid Sp. gr. 1.305.	Number of Divisions of the Alkalimeter of Water added to the Acid.	Sp. gr. of the Acid.	Sulphate of Potash dissolved in the Acid.	Nitrate of Baryta poured in.	Time required for a Precipitate to appear.
1	20	20	1.167	3.34	0.062	No precipitate after 24 hours in any of the jars.
2	20	40	1.120	......	......	
3	20	60	1.085	......	......	
4	20	80	1.067	.....	......	
5	20	100	1.057	......	......	
6	20	120	1.050	......	......	
7	20	140	1.044	......	......	
8	20	160	1.039	......	......	
9	20	180	1.035	......	......	
10	20	200	1.032	......	......	

HAS THE FORMATION OF A PRECIPITATE AN INFLUENCE ON THE SOLUBILITY OF SULPHATE OF BARYTA.

It is generally believed that the formation of a precipitate tends to increase its quantity, but if this opinion be correct with regard to crystalline precipitates, such as those of bitartrate of potash, carbazotate of potash, &c., it is *not* so with sulphate of baryta; for it will be seen, in the following table, that the quantity of sulphate of baryta which remains dissolved in 1000 grs. of nitric acid, sp. gr. 1.167, at 60° Fah., is the same, whether there is or is not a precipitate formed in the liquors.

Order of Table.	No. of Jar.	Quantity of Acid Sp. gr. 1.167 employed.	Weight of Sulphate of Baryta dissolved.	Quantity of Acid.	Weight of Sulphate of Baryta dissolved per 1000 grs.	Remarks.
2	5	2334.0	4.46	1000	1.957	With no precipitate.
3	4	1867.2	3.66	...	1.911	With a precipitate.
4	8	3734.0	7.136	...	1.919	With no precipitate.
5	5	2334.0	4.806	...	2.059	With a precipitate.
Results with 15 grs. $BaO SO_3$	9	4201.2	9.130	...	2.066	ditto
	10	4668.0	10.470	...	2.149	ditto

ON THE INFLUENCE OF MASS ON CHEMICAL AFFINITY.

When the results contained in the preceding tables are compared, there is observed a certain number of facts which demonstrate the influence of masses of matter "put in presence;" and in order to corroborate this assertion, I add here a certain number of results, extracted from the preceding tables.

1. If we compare the results given by the two first jars in Tables Nos. 1, 2, and 4, it will be seen that, although the same quantity of aeid was employed, the time necessary for the formation of a precipitate varied considerably.

Order of Table.	Quantity of Acid employed.	Weight of Sulphate of Potash.	Weight of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a Precipitate appeared.
1	466.8	0.753	1.121	1.00	12 hours.
	933.6	0.753	1.121	1.00	none.
2	466.8	3.34	5.00	4.46	instantly.
	933.6	3.34	5.00	4.46	2 hours.
4	466.8	5.12	8.00	7.13	instantly.
	933.6	5.12	8.00	7.13	2 minutes.

The differences observed above can only be owing to the fact that, for the same quantity of fluid, increasing proportions of nitrate of baryta and sulphate of potash were employed; for it cannot be explained by admitting that it is owing to there not being a sufficient quantity of nitric acid to hold a greater quantity of sulphate of barytes in solution; for if we calculate how much sulphate of baryta remains dissolved in 1000 grs. of acid, it will be seen that the quantities are the same in each of the three tables.

Order of Table.	Order of Jars.	Quantities of Acid employed.	Quantities of Fluid represented by.	Weight of Sulphate of Baryta dissolved.
1	1	466.8	1000	2.142
2	4	1867.2	...	2.369
	5	2334.0	...	1.911
4	7	3267.6	...	2.182
	8	3734.4	...	1.909

Neither can it be admitted that the difference observed is owing to the formation of a precipitate, for I have shown above that the presence of a precipitate has not any influence on the degree of solubility or non-formation of sulphate of baryta.

2. The increased solubility of sulphate of baryta, arising from the presence of quantities of matter, can only be explained by admitting that this solubility is influenced by the increased quantities of salt employed, since the volume of acid is the same in every case, and all other circumstances are equal.

Order of Table.	Order of Jar.	Quantity of Acid employed.	Quantity of Sulphate of Baryta dissolved.	Quantity of Sulphate of Baryta dissolved per 1000grs.	Increased ratio of solubility per 1000grs. of Acid.
1	2	933.6	1.00	1.072	
2	6	2800.8	4.46	1.522	.450
4	9	4201.2	7.13	1.912	.390

I will again introduce the figures given above, as they show in a striking manner the rapid increase of the solubility of sulphate of baryta, for 1000 grs. of acid, in three successive jars of the same series.

Order of Table.	Order of Jar.	Number of Divisions of the Alkalimeter.	Corresponding Weight of Acid Sp. gr. 1.167.	Weight of Sulphate of Baryta precipitated.	Weight of Sulphate of Baryta dissolved.	Weight of Sulphate of Baryta dissolved per 1000grs.
1	1	40	466.8	4.44	0.02	0.043
	2	80	933.6	3.17	1.29	1.382
	3	120	1400.4	2.02	2.34	1.670
	4	160	1867.2	0.80	3.66	1.957

Therefore it appears to me that we must admit that the difference observed is due to the quantities of matter "put in presence."

I would also, in conclusion, call attention to the enormous differences between the effects of multiple volumes of an acid, compared with volumes of the same acid decreasing in density.

ON THE SOLUBILITY OF SULPHATE OF BARYTA IN HYDROCHLORIC ACID.

I thought it advisable to repeat some of the above series, substituting hydrochloric acid for nitric acid, and it will be seen that although sulphate of baryta is less soluble in this acid than in nitric acid, still it is sufficiently so, to cause chemists to avoid its use in excess in delicate analyses.

The following table shows the results obtained by employing multiple volumes of hydrochloric acid, of sp. gr. 1.0775, with quantities of nitrate of baryta and sulphate of potash, capable of producing 1 gr. of sulphate of baryta.

TABLE No. 12.

Order of Jar.	Divisions of the Alkalimeter of Acid Sp.gr.1.0775	Weight of Sulphate of Potash.	Weight of Nitrate of Baryta.	Weight of Sulphte. of Baryta.	Weight of Sulphate of Baryta obtained instead of 1.00	Weight of Sulphate of Baryta dissolved.	Time when a Precipitate appeared.
1	40	0.735	1.121	1.00	.96	.04	1¾ minute.
2	80	.....	......	......	.85	.15	9 minutes.
3	120	.....	......	......	.83	.17	50 minutes.
4	160	......	......	......	.66	.34	3¾ hours.
5	200	......	......	......	.48	.52	10 hours.
6	240	.....	......	......	.22	.78	24 hours.
7	280	......	......	......	no precip.	1.00	no precipitate.
8	320	......	......	......	......	......	......
9	360	......	......	......	......	......	.....
10	400	......	......	......	......	......	......

On comparing the results contained in this table with those of No. 1 Table of the nitric acid series, it will be seen that instead of the non-formation of a precipitate in jar No. 2, one was produced in the first six jars; sulphate of baryta, therefore, is less soluble in hydrochloric acid than in nitric acid, and still if we calculate, taking for term of comparison the results obtained in jar No. 7 of hydrochloric acid, it will be found that its solubility is equal to one part of sulphate of baryta in 2800 parts of hydrochloric acid; and consequently the solubility of sulphate of baryta in hydrochloric acid, sp. gr. 1.0775, is fifty times greater than in distilled water.

I have also made a series, employing the same quantities of salt of potash and of baryta, as in series No. 2 of nitric acid.

TABLE No. 13.

Number of Jar.	Divisions of the Alkali-meter of Acid Sp.gr.1.0775	Weight of Sulphate of Potash.	Weight of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Weight of Sulphate of Baryta obtained instead of 4.46.	Weight of Sulphate of Baryta dissolved.	Time when a Precipitate appeared.
1	40	3.34	5.00	4.46	4.35	.11	instantly.
2	80	......	......	.....	4.34	.12	instantly.
3	120	......	......	......	4.24	.22	20 seconds.
4	160	.....	......	......	4.20	.26	1¼ minute.
5	200	......	......	......	4.14	.32	2 minutes.
6	240	......	......	......	4.14	.32	3½ minutes.
7	280	......	......	......	4.17	.29	6 minutes.
8	320	......	.....	......	3.94	.52	9 minutes.
9	360	......	......	......	3.98	.48	13 minutes.
10	400	......	......	.....	3.96	.50	18 minutes.

I have also made a series of experiments, employing no longer multiple volumes of the same acid of a constant strength, but multiple volumes of acid decreasing in strength, and it will be observed that the results in this last table are comparable with those of Table No. 7 of the nitric acid series.

TABLE No. 14.

No. of Jar.	Divisions of the Alkali-meter of Acid Sp. gr. 1.1425	Divisions of Water added.	Weight of Sulphate of Potash.	Weight of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Weight of Sulphate of Baryta obtained instead of 4.46.	Weight of Sulphate of Baryta dissolved.	Time when a Precipitate appeared.
1	20	20	3.34	5.00	4.46	4.31	Average	instantly.
2	20	40	...	...	...		quantity dis-	instantly.
3	20	60	...	...	...		solved equal	instantly.
4	20	80	...	...	...	4.31	to 0.15.	15 seconds.
5	20	100	...	...	...			ditto
6	20	120	...	...	..	4.32		ditto
7	20	140	...	...	...			20 seconds.
8	20	160	. .	...	...			ditto
9	20	180	...	...	...			ditto
10	20	200	...	..	...	4.35		ditto

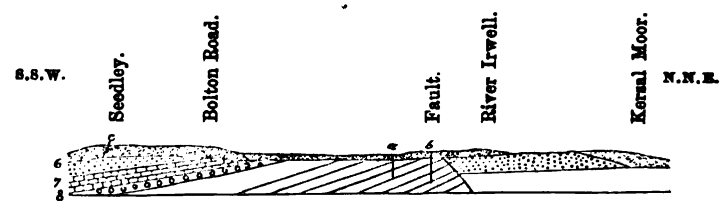
VII.—*Additional Observations on the Permidn Beds of the North-west of England.*

By E. W. Binney, V.P.; F.R.S., F.G.S.

[*Read 24th February*, 1857.]

In a former communication made to this society on the permian beds of the north-west of England, and printed in vol. XII. (new series) of its Memoirs, I have alluded to the fragmentary condition in which our knowledge on this subject was. I then published what information I had obtained as an outline, trusting that other parties better qualified for the task would fill up the vast gaps that I had passed over. Since that time I have collected some further information on the subject at Seedley and Astley, near Manchester; Irby Hall, near Kirby-in-Lonsdale, and Howrigg, Shawk and Westward, near Wigton, Cumberland. This I now hasten to lay before the society, in the hope that it may probably induce other labourers to work on the permian deposits of this and the neighbouring districts.

SEEDLEY SECTION.



Middle and Upper Coal Field.
a and b. Mr. Fitzgerald's old and new coal pits.
c. Seedley bore hole.
6. Trias.
7. Red marls and limestones.
8. Conglomerate.

At page 231, in the paper before alluded to, under the head of "Pendleton," there was given a section of the strata lying between Oatbank, on the Eccles New Road, and Kersal Moor, both well known places in the immediate vicinity of Manchester, and lying to the north and west of the city. In that section, the upper new red sandstone, the lowest member of the trias, was laid down; but the permian deposits, although supposed to lie under the drift between Oatbank and the Bolton New Road, north-west of Pendleton, were not put in, no direct evidence of their occurrence in that part having been then obtained.

Mr. Appleby, of the Seedley Printing Company, has lately made a bore hole at their works in Seedley, for the purpose of increasing the supply of water there. In the spring of 1856 he called upon me for the purpose of ascertaining the character of the deposits lying under the upper new red sandstone, in which rock he had already several bore holes. I informed him that it was pretty certain that the upper permian beds, namely, the red marls with thin beds of limestone, would be about 130 feet in thickness; but the inferior portion, namely, the conglomerate and lower new red sandstone, were rocks of so variable a nature that it was impossible to say how thick they might be—whether 6 feet or 600 feet. Mr. Appleby has since

favoured me with a copy of the bore, and samples of the strata gone through. The hole was 15 inches in diameter, and was made by Messers. Mather and Platt, of Salford, with their new apparatus for boring. On the whole work the workmen, I am informed, averaged from eight to nine feet a day, and during their passage through the upper new red sandstone, they generally went three feet per hour;—certainly a very extraordinary speed for such a bore. The following is a list of the beds gone through:—

	Ft.	In.		
Till	61	0	Drift.	
Soft red sandstone with a few small rounded quartz pebbles in it	139	0	Trias.	
Very tenacious red clay	8	0	Red marls and thin beds of limestone	Permian.
Hard red raddle, very close in texture	0	6		
Red clay and red gritty sandstone	1	6		
Red clay, red gritty sandstone, and whitish gray rock, the latter soft	6	0		
Red clay and beds of stone	16	0		
Ditto	28	0		
Red clay	4	0		
Hard white rock	2	0		
Red clay with thin beds of stone	4	0		
Ditto	59	6		
White rock	0	6	Conglomerate.	
Red sandstone with beds of raddle	11	6		
Bluish-white stone, rather soft	0	6		
Clay with shale containing coal plants	30	0	Carboniferous	
	372	0		

Now, in the above section the upper permian beds, namely, the red clays (marls) with thin beds of limestone, are 128 feet in thickness—just about what was expected; but the lower beds, which, when last seen at Collyhurst, two miles and a-half to the south-east, were 350 feet in thickness, and at Patricroft, on the west, within one mile and a-half of Seedley, were 21 feet, only proved to be 12 feet 6 inches thick. These beds bear no resemblance to the soft crumbling sandstone at Heaton Mersey, Beet Bank Bridge, and Collyhurst, but are

like the conglomerates of Cheetham weirhole and Patricroft; and I am, therefore, inclined to class them as such. They are, I believe, as stated in my former paper, at page 266, often unconformable to the underlying lower new red sandstone upon which they rest.

The clay, with beds of shale, 30 feet in thickness, was good coal measures, as the lighter coloured beds were full of the rootlets of *Stigmaria ficoides*.

The occurrence of coal measures at this place is of considerable importance, as they were met with at a depth of only 115 yards from the surface, and if a good seam of coal could be found the field is of tolerable extent, and, from its position, would certainly command a first-rate market. The strata which I examined afforded no evidence of what part of the coal-field they belonged to; but, from the position of the Pendleton seams on the north-east, and the Patricroft ones on the west, they probably belong to the upper coal-field. The most likely seam to be found is the Openshaw coal, formerly worked near the Water Works Reservoir, in Beswick, near Manchester, and occupying a geological position near to the Slack Lane coal, formerly worked by Messrs. Lancaster at Monton.

Astley Section.

At pages 235 and 268 of my former paper, I gave particulars of a bore hole made by G. P. Kenworthy, Esq., in a field called the Horse Pasture, in Astley. In the upper permian beds there, fifty-five beds of limestone had been met with in a distance of 230 feet 6 inches. Many of these contained fossils of the genera *Schizodus*, *Bakevellia*, and *Tragos*. By the kindness of my friend, H. Mere Ormerod, Esq., I am enabled to add the following additional particulars, which carry the section through the lower permian beds into the coal measures.

	Ft.	In.	
Brown rock with water	10	0⅛	
Ditto	6	8¼	
Ditto	5	4⅜	
Ditto burr	1	5¼	
Ditto soft	0	10½	
Ditto hard	10	8	
Whitish rock	1	0	
Brown-red rock	4	5	
Hard burr	1	1½	
White sandstone	4	11	
Irony burr	0	6	
Brown-red rock	2	7	
Red metal	0	5½	
Brown-red rock	34	6¼	
Hard burr	2	2	
Soft bed	0	1	
Hard burr	1	0	Lower Permian.
Brown-red rock	0	7	
White or gray rock	2	1	
Brown and red rock burr	1	2½	
Ditto	3	0	
Ditto	7	10½	
Bright red rock	11	6⅓	
Burr	0	10⅓	
Bright red rock	3	0	
Ditto very hard	4	4⅜	
Red ironstone burr	1	6	
Bright red rock	11	7	
Red burr	0	6	
Red rock or linsey	6	0	
Red rock with clay	2	8	
Red burr	2	4	
Red rock	5	2	
Red raddle	0	9	
Red rock	0	11	
Blue metal	3	0	
Black shale	1	4	
Red metal	7	0	
Black shale	0	10	
Linsey	0	7	
Blue metal	3	1⅓	
White rock	0	7	
Black shale with ironstone bands	3	7	
Rocky warrant	4	7	
Red and blue metal	4	4	
Black shale	0	10	

	Ft.	In.	
Blue metal	9	5½	Carboniferous.
White rock	0	5	
Blue metal	1	1½	
Strong white rock	0	9	
Blue metal	1	0	
White rock	0	5	
Blue metal	1	3	
White rock	3	0½	
Dark blue metal	5	8	
Strong white rock	1	8	
Blue metal	1	1	
White rock	0	4	
Blue metal	6	2	
Whitish blue metal	7	6½	
Black bass	0	2½	
Smut of coal	0	1½	
Warrant	3	11	
White rock	7	2½	
Linsey	6	1	
White rock	7	5	
Blue metal	4	0	
Black shale	0	3½	
White rocky warrant	5	2	
Hard white burry rock	0	6	
Light gray metal	0	8	
Light blue metal	10	5	
Black shale	0	3	
Light gray metal	4	0	
Gray linsey	1	9	
Light blue metal	4	4	
Gray linsey	3	11	
Strong white rock	0	11	
Gray linsey	0	10	
White rock	2	5	
Gray metal or linsey	0	3	
White rock	2	10	
Gray burr, very hard	2	1	
White rock	1	1½	
Burr	0	11½	
Parting of blue metal	0	3	
White rock	0	8½	
	522	8	

This section gives a thickness of 150 feet $2\frac{3}{4}$ inches for the lower permian beds.* The coal measures appear to belong to the upper field, and although 153 feet 7 inches were penetrated, no seam of coal was met with.

By the further kindness of Mr. H. Mere Ormerod I have been furnished with a copy of the strata met with by Messrs. Sam Jackson and Co., in sinking their new pit down to the Four Feet seam of coal at Bedford Lodge. This working lies a little to the south of the places where the permian beds described by Mr. G. Wareing Ormerod, M.A., F.G.S.,† were met with, and not a great distance on the rise from Mr. Kenworthy's bore hole, previously alluded to in this paper. The dip of the strata is slightly east of south, at an angle of 9°; and as this is one of the best sections of the permian beds ever met with in Lancashire, I shall give it at length:—

	Yds.	Ft.	In.	
Soil	0	0	10	Drift.
Yellow clay and marl	1	0	2	
Dry lum and gravel	0	2	8	
Fine sand, very wet	0	1	0	
Lum and gravel, very wet	1	0	10	
Gravel, wet	0	1	4	
Sand, wet	0	1	0	
Fine gravel, wet	0	2	0	
Soft red rock	1	2	6	Trias.
Soft red rock band and gray rock	2	0	4	
Soft red rock with gray bands	3	0	6	
Yellow rock	0	0	9	
Soft red metal	0	1	6	
Red metal, rather stronger	2	1	7	
Fine light blue metal	0	0	1	
White gritty rock like bone or a bed of magnesia	0	0	1	
Soft red metal	6	2	0	
Soft white rock	0	0	8	
Red metal	0	1	4	

* This is about the thickness of the rock at Edge Green. See page 243 of the author's former paper.

† Quarterly Journal of the Geological Society of London, Vol. VII., page 268.

	Yds.	Ft.	In.
Soft red metal	0	2	6
Soft gray rock	0	0	8
Red metal	0	1	10
Gray rock	0	0	10
Red metal	2	2	6
Soft blue metal	0	0	2
Hard bastard limestone	0	0	3
Soft red metal	1	1	8
Red metal, rather stronger	0	2	4
Red stone bind or floor	1	0	6
Red stone bind, softer	1	2	2
Light blue metal	0	0	1
Soft red metal	1	0	7
Light blue metal	0	0	1
Red metal flooring	0	2	2
Red metal	0	1	0½
Light blue metal	0	0	2
Red metal	0	1	3½
Stony blue rock	0	0	5
Red metal	2	1	4
Blue metal	0	0	2
Red metal	0	0	6
Blue ditto	0	0	2
Red ditto	0	0	2
Blue ditto	0	0	2
Red ditto	0	0	4
Top band of limestone	0	0	5
Red metal	0	0	4
Blue metal	0	0	1
Red ditto with streaks of blue	0	1	4
Blue ditto with streaks of red	0	0	5
Red ditto	0	0	4
Blue ditto	0	0	2
Mainband limestone	0	0	11½
Red metal	0	0	2½
Limestone	0	0	7
Blue metal	0	0	1
Red ditto	0	0	1½
Blue ditto	0	0	1½
Red ditto	0	0	2
Limestone	0	0	1½
Red metal	0	0	1½
Limestone	0	0	2
Blue metal	0	0	1
Red ditto	0	0	1
Blue ditto	0	0	0½

	Yds.	Ft.	In.	
Red metal	0	0	4¼	Upper Permian. Red and variegated marls and limestones.
Blue ditto	0	0	1	
Red ditto	0	0	1	
Limestone	0	0	4	
Red metal	0	0	0½	
Limestone	0	0	1¼	
Red metal	0	0	2½	
Limestone	0	0	8½	
Red metal	0	0	0½	
Limestone	0	0	1	
Red metal	0	0	1	
Limestone	0	0	5	
Red metal	0	0	2½	
Limestone	0	0	4	
Blue metal	0	0	3	
Red ditto	0	0	4	
Limestone	0	0	3	
Red metal	0	0	0½	
Limestone	0	0	1½	
Blue metal	0	0	1½	
Red ditto	0	0	2½	
Limestone	0	0	1½	
Red metal	0	0	4½	
Limestone	0	0	4	
Red metal	0	0	2	
Limestone	0	0	1	
Red metal	0	0	4½	
Limestone	0	0	1	
Red metal	0	0	1	
Limestone	0	0	2	
Red metal	0	0	3	
Limestone	0	0	1	
Red metal	0	0	4½	
Bottom limestone band	0	0	3	
Blue metal	0	0	1	
Red metal with blue streaks	2	0	0	
Red metal	0	1	0	
Bastard limestone band	0	0	1	
Red metal	0	0	7	
Bastard limestone band	0	0	1	
Red metal	0	0	7	
Bastard limestone band	0	0	1	
Red metal	0	0	6	
Bastard limestone band	0	0	1	
Red metal	0	0	6	
Limestone balls	0	0	2	

	Yds.	Ft.	In.
Red metal	0	0	8
Limestone	0	0	1
Red metal with limestone balls	1	0	10
Red ditto ditto	0	2	6
Red ditto	3	1	6
Blue ditto	0	0	2½
Soft blue rock	0	0	2½
Soft red rock	1	1	5
Ditto	1	2	0
Red metal	0	0	3
Light blue ditto	0	0	0½
Red metal	0	0	1
Red rock	0	0	1
Red metal	0	0	1½
Red rock	0	0	5
Soft red metal	0	0	6½
Red rock	0	0	1½
Red rock with streaks of metal	0	1	4
White rock	0	0	11½
Red metals	0	0	8½
Gray rock, sandy, full of joints, yields much water	3	2	7
Soft gray rock, mixed with bands of dark gray metals	0	1	9½
Gray sandy rock floor or parting	2	1	0
Ditto ditto	1	1	5
Dark gray sandy rock, mixed with white hard pebbles and red raddle balls	5	1	10
Hard sandy rock	1	2	10
Dark brown metals	1	2	8
Metals or linsey, mixed with rock bands	1	0	5
Ditto ditto	1	1	9
*Red metals	6	2	6
Black bass or shale	3	0	10
Floor or warrant, hard	1	1	1
Gray bass or shale	0	2	0
Black bass or shale	0	1	11
Brown metals, mixed with ironstone balls	0	2	5
Ditto mixed with gray rock, soft	1	1	1
Soft gray metals	0	1	4
Linstey, mixed with rock bands	3	0	6
Blue metals, soft	2	0	0
Dark metals, blackish cast	0	1	8
Metals, lightish colour	0	2	3
Black shale and bastard coal	0	1	8
Warrant, dark colour	0	1	9

Lower permian beds containing fossil plants.

	Yds.	Ft.	In.	
Linstey, mixed with rock bands	2	0	3	Carboniferous.
Soft flaggy rock	0	0	5	
Blue metals, mixed with ironstone bands or bullions	3	1	8	
Black shale	0	0	9	
Warrant, rocky	0	1	0	
White rock, strong, with bands of metal...	3	0	2	
Rock, soft and shaly	0	2	8	
Strong gray metals or linstey	2	2	2	
Strong metals	1	1	3	
Coal	0	0	3	
Strong rocky warrant	1	2	3	
Light blue metals	0	2	6	
Dark rock or burr	0	1	0	
Black bassy coal	0	0	10	
Strong warrants with streaks of metal	1	0	8	

The upper part of the permian beds named in the above section, consisting of red marls and limestones, contains the usual permian fossils, consisting of shells of the genera *Schizodus*, *Bakevellia*, *Turbo*, and *Rissoa*, and a sponge of the genus *Tragos*.

The number of beds of limestone is only 26, whereas in the bore at Messrs. Kenworthy's 54 were noticed. In the latter case, most of the hard beds were put down as limestone, whilst in the former, only those which appeared to the workman to be limestone were noticed as such; and sometimes two or three beds were classed as one. In the former also several beds were enumerated containing limestone balls, which in the latter were put down as separate beds. These circumstances may, in some measure, account for the great difference in the number of beds.

In my former paper, at page 238, I alluded to the occurrence of red and variegated sandstones containing impressions of *Calamites* and *Sigillaria*, and pieces of fossil wood, which had been found in sinking the pit at the Bedford Colliery some years since; but whether these fossils belonged to the carboniferous or permian strata—as both were lying on the pit bank

mixed together—I could not undertake to speak with certainty. If, however, they belonged to the last-named strata, they were the only specimens of fossil plants that had then to my knowledge been met with in the permian beds of the north-west of England. Now, in sinking the shaft at Bedford Lodge, of the strata in which I have given full particulars above, I collected undoubted specimens of *Sigillaria*, *Lepidodendron*, and *Sternbergia*, the specific characters of which could not be distinctly recognised, *Calamites cannæformis* and *C. approximatus*, and fossil wood, both in the fine-grained sandstones and coarse conglomerates containing rounded and partly rounded pebbles of white quartz, of the size of a common marble. The fossils occurred in all the arenaceous rocks found under the red marls and limestones, and lying above the carboniferous strata. All the beds were full of nodules and patches of a soft liver-coloured clay containing iron, and most of the fossil wood was converted into that substance. The strata which contained the fossil plants I take to be permian from their geological position, as well as from the pebbly character of the beds; for, although many of the red sandstones of the upper coal-field of Lancashire doubtless contain much red oxide of iron, and red and liver-coloured nodules of clay, I never yet found a coal measure sandstone, except in the lower field, containing large quartz pebbles;—in fact, some of these beds are quite as much conglomerates as the millstone grits are, and can scarcely be distinguished from those rocks, except that they are rather more pulverulent. The men engaged in sinking did not know where the permian strata ceased and the coal measures commenced, for both strata dipped in the same direction, and nearly at the same angle.

The coal measures to a stranger would appear to pass into the overlying permian strata, but the Worsley and Astley Four Feet seam lying under unquestionably proves that the former beds, at their point of contact with the permian strata,

are 1,100 feet down in the series from the highest beds of the upper coal-field, and, therefore, really belong to the lower part of that division.

In the red metals marked with an asterisk, 6 yards, 2 feet, 6 inches in thickness, specimens of *microconchus carbonarius*, a *cypris*, and fish scales were met with; thus clearly proving that the carboniferous strata had there commenced. In fact, these strata lie a short distance above the Four Feet coal which has been already worked under the spot where the shaft was being sunk.

Irby Hall Section.

At page 262 of my former paper, a description is given of the permian beds found at Westhouse, near Ingleton. Since the time when that sketch was written, I have met with another section of permian beds, exposed in the railway cutting near Irby Hall, lying between Westhouse and Kirby Lonsdale, about three miles from each of those places.* The breccia or conglomerate is similar in appearance to that at Westhouse, is not exposed for more than about 200 yards, and is capped by a deposit of 20 feet of brownish-coloured till. Its position with regard to the underlying rocks of the district cannot be seen. It dips to the north-east at an angle of 12°, but as you proceed to the west under the railway bridge the dip increases to 25°. The rock is composed chiefly of limestone pebbles, varying from the size of a marble to that of a man's head, some being round and others angular, cemented together by a reddish-coloured clay. Besides other stones, consisting of slates, silurians, and old red sandstones, rounded pebbles of milk-white quartz, like vein quartz, are found.

* I am indebted to Mr. Hindson, of Kirby Lonsdale, for taking me to this locality, and showing me the section.

About two years ago, Sir Charles Lyell, M.A., F.R.S., when in Manchester, showed me some breccias which he had lately collected in the vicinity of the Shropshire coal-field. In these specimens was a cream-coloured limestone, somewhat resembling a common mountain limestone, except that it had a porcelain-like appearance, possessed a slightly conchoidal fracture, and contained no fossil organic remains. This rock I immediately recognised, merely from its external characters, as one of the so-called freshwater* limestones of the upper part of the coal-field, like those of Leebotwood and Uffington, near Shrewsbury; Lane End, Staffordshire; Ardwick, near Manchester; and Whiston, near Liverpool; and I then remarked that it had not travelled far, but had been derived from the neighbouring coal-field.

Now, in the specimens of limestone found in the breccia at Irby Hall, some of them have been a good deal decomposed by water containing carbonic acid gas in solution, and many of the fossil corals have been thus brought out of their matrices and exhibited in relief. The *Syringopora ramulosa* and other corals I have collected. But the most interesting specimens from this locality are some pieces of an upper carboniferous limestone, like those before mentioned, and no doubt derived from a portion of the neighbouring Black Burton coal-field, containing undoubted specimens of the *Microconchus carbonarius* and *Cypris inflata*, as well as pieces of a peculiar flinty-looking chert which I have not hitherto met with any where in the north-west of England, except at Ardwick.

The occurrence of a carboniferous rock is what might have been expected from the vicinity of the neighbouring coal-field, since the rocks of the permian breccia or conglomerate, as I

* See papers by the author On the Origin of Coal, vol. VIII. (second series), pages 157 and 185, and On some Trails and Holes found in Rocks of the Carboniferous Strata, vol. X., page 199, of the Society's Memoirs, where he gives his reasons for the marine character of the strata of the coal measures.

have before stated, generally vary with the description of the older rocks found in the immediate vicinity; but such a limestone as the porcelain-like one is at the present time no where to be met with in the Black Burton coal-field. This rock would most probably, as in the southern coal-fields of Lancashire, be found in the upper part of the formation, but as this portion of the strata has here been removed before the breccia or conglomerate was formed, these interesting fragments afford evidence of the former occurrence of a rock now no longer to be met with *in situ*, and thus supply us with another proof of the great denuding action which took place at the close of the carboniferous epoch.

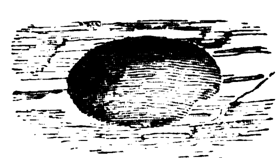
Howrigg Section.

In a geological map of the Lake Districts, published by Mr. John Ruthven, of Kendal, in 1855, a small tract of what he terms magnesian limestone is laid down. It is about two miles in extent from east to west, and lies a little south of Howrigg, near Wigton. In the month of October last I went over to Howrigg, and although I did not meet with any true permian magnesian limestone, certainly some of the mountain limestones there found might very easily be mistaken for such were it not for their fossil organic remains; and these are by no means abundant, or commonly to be met with. I, however, saw a small patch of permian breccia, and some fine-grained sandstones of a brick-red colour, containing very beautiful ripple marks, and, in their lower portions, desiccation cracks, which very much reminded me of the permian sandstones found at the Craigs, near Dumfries. Like the latter, some of the beds are so finely laminated as not only to be used for flags, but also for slating houses.

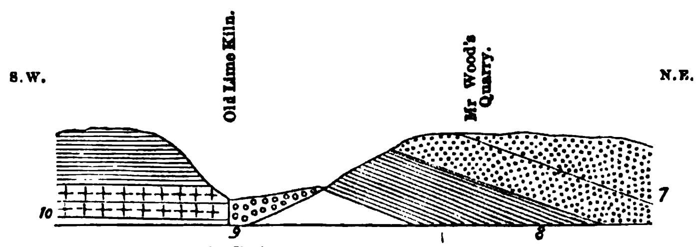
On approaching the district from Carlisle, I reached the Curthwaite station, on the Maryport railway, but saw nothing

of the strata there, owing to the thick covering of drift deposits between Ourthwaite and Howrigg. On the top of the hill, near the last named place, is a small sandstone quarry, on the south side of the road. The stone there met with is laminated, of a brick-red colour, variegated with brownish tints, and divided by several red clay partings. It contains many desiccation cracks and ripple marks, and the dip of the strata is about 20° west of north, at an angle of 15°.

A little to the south of the last named place is Howrigg quarry, worked by Mr. Wood. This is an extensive excavation of fine-grained laminated sandstone, of a brick red, containing beds of a greenish-brown colour. It affords stones with many beautiful ripple marks on their surfaces, and desiccation cracks in some of the lower beds. In its lines of bedding the stone frequently shows considerable traces of black oxide of manganese, and some of the lower strata are of a greenish-brown colour, and contain nodules of greenish clay. Altogether 30 to 40 feet of stone must be exposed in the quarry. The dip of the strata is towards 20° west of north, at an angle of 15°. I looked diligently for tracks, but only saw one specimen at all likely to be taken for such, and this I unfortunately broke in attempting to cut it out with a chisel. It was a single track, and occurred in the light-coloured sandstone near the bottom of the quarry, and I traced seven impressions, each about an inch and a half apart, and one-sixth of an inch in depth, all running in nearly a straight line. The accompanying wood cut represents one of the impressions.



SHAWK SECTION.



7. Red and variegated sandstones.
8. Red and shaly marls.
9. Conglomerate.
10. Mountain limestone.

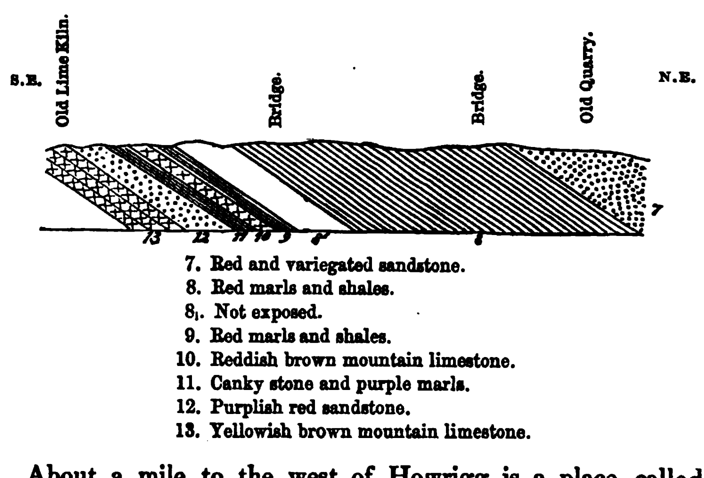
I next examined the large sandstone quarries of Mr. Wood, known by the name of the Shawk quarries, extending from Shawk Foot to the hill opposite the old lime kilns. They consist of fine-grained laminated sandstones of a brick-red colour, and contain variegated beds, desiccation cracks, and ripple marks, and in all respects resemble the Howrigg stone, before described, of which they are no doubt a continuation. The dip of the strata is to the north-east, at an angle of 15°, and probably not less than 200 to 300 feet in thickness of stone may be exposed. This sandstone is succeeded on its rise by about 140 feet of red shaly marls, dipping a little east of north, at an angle of 15°. Then comes in bottom of the valley, near the brook, but I could not trace it going under the red marls, a bed of breccia. This is composed of angular pieces of mountain limestone, cemented together by a paste of reddish clay containing more or less of red sand. It is seen below the lime kiln, near the brook side, and the beds occur in the following descending order:—

Reddish and light-coloured sandstone.........7 feet.
Brown breccia ....4 ,,

The dip of the strata is to the south-west, at an angle of 18°. This is in the opposite direction to the dip of the sandstones and clays. Within a few yards of the breccia the mountain

limestone is quarried. It is of a cream colour, and partly crystalline. The dip, if any, is to the north-east, but it lies nearly level. Probably 30 feet in thickness of the limestone may be exposed. In it I found portions of a large *Producta*, a *Cyathophyllum*, and some crinoidal stems. Between the breccia and the mountain limestone is a fault, running from south-east to north-west, and this dislocation may have altered the inclination of the former rock, and thus caused it to appear to dip near the fault in a contrary direction to the red sandstones and clays, under which, probably, at a distance from the fault, it may dip and be conformable with. Of this, however, I at present cannot speak positively, as I have not seen the breccia underlying the red clays.

WESTWARD SECTION.



7. Red and variegated sandstone.
8. Red marls and shales.
8_1. Not exposed.
9. Red marls and shales.
10. Reddish brown mountain limestone.
11. Canky stone and purple marls.
12. Purplish red sandstone.
13. Yellowish brown mountain limestone.

About a mile to the west of Howrigg is a place called Westward, and in the small brook course there is exposed a yellowish-brown limestone, partly earthy and partly crystalline in structure, very much resembling the ordinary permian magnesian limestone of Yorkshire and Durham. It has been

quarried to some extent for the purpose of making lime, but, from the quantity of unused stone left in the quarry, the attempt does not appear to have proved very successful. It dips 15° west of north, at an angle of 15°. I observed crinoidal stems in it, but no other fossils. This limestone is succeeded by a purplish-red coloured sandstone, much laminated, and containing many worm holes, similar to those of the *Arenicola carbonaria*, and some singular contorted worm coils and tracks. In all its characters this sandstone so much resembles the rock termed Smardale sandstone,* and found low down in the mountain limestone at Smardale, and in the bed of the Belah at Whitrigg, near Kirby Stephen, that I have no hesitation in pronouncing it to be of the same geological age. Then come beds of canky stone and purple-coloured marls. These are succeeded by a bed of hard limestone of a cellular structure and a reddish-brown colour, very much resembling the permian limestones of South Lancashire, and not to be distinguished from similar beds of mountain limestone found in the bed of the Belah. In this limestone I detected crinoidal stems. Next came red marls and shales, with a short distance on the bank of the stream covered up with drift, so as not to be visible. Up to this point, no doubt, the strata are carboniferous, but here commence a series of red marls, containing circular concretions of a greenish colour, with a black spot in the middle, parted by sandy beds, some of which contain ripple marks. These strata occupy the brook course between the two bridges, and exactly resemble some beds seen in the river Ayr, near Catrine, in Ayrshire, and on the beach near Methill Harbour, on the coast of Fife. Although these deposits have as yet afforded no fossil remains, I am inclined to regard them as permian rather than carboniferous. Next comes a laminated sandstone, fine-grained,

* See Professor Sedgwick's paper On the Lower Palæozoic Rocks at the base of the Carboniferous Chain between Ravenstone Dale and Ribblesdale. Quarterly Journal of the Geological Society of London, Vol. VIII., page 43.

and of a brick-red colour, dipping to the north-west at an angle of 25°. This stone I cannot distinguish from the sandstones of Howrigg and Shawk, previously described.

The beds lying between the most southerly bridge over the brook and the old lime kilns are certainly more like permian than carboniferous beds in their appearance, and might be taken for the former, as similar beds in the Belah have been by experienced geologists, if it were not for the fact that both the Westward and Belah deposits contain crinoidal stems, and other undoubted mountain limestone fossils.

The beds in this section—namely, the red marls and sandy deposits—which appear to me to belong to the permian group, dip at the same angle and in the same direction as the underlying carboniferous strata, namely, at an angle of 15°, but the red sandstone under which they disappear, although dipping in the same direction, inclines at an angle of 25°, instead of 15°.

The brick-red sandstones of Howrigg, Shawk, and Westward, with their underlying red clays, as well as the breccia at Shawk, I have little doubt will be proved to be permian. It is true that no fossil organic remains have yet been found in them, with the exception of the track alluded to in this paper; but if mineralogical characters and geological superposition are to be taken as evidence of their age, they are as good permian beds as those of Westhouse, Kirby Stephen, and Brough, in England, and Dumfries and other places in the south-west of Scotland, with the latter of which they are most probably connected.

VIII.—*The Chemical Changes which Pig Iron undergoes during its conversion into Wrought Iron.*

By F. Crace Calvert, F.C.S., M.R.A. Turin, and Mr. Richard Johnson.

[*Read March* 10*th*, 1857.]

Wishing to make some improvements in the manufacture of iron, we carefully examined the various analyses which had been made of pig and wrought iron, and we found that no comparison could be drawn between the results, as the samples analysed had been obtained from different sources; and also that no detailed analysis had been published of the various chemical changes which pig iron undergoes in puddling during its conversion into wrought iron. We therefore at once decided to undertake this task, with the hope of throwing some light upon this important operation. Fully to investigate the progressive and interesting chemical changes which cast iron undergoes during its conversion into wrought iron, we took samples every five or ten minutes after the pig iron had melted in the furnace. These chemical actions are clearly defined in the furnace by the peculiar appearance which the mass assumes as the operation proceeds. Before describing the various chemical changes, the appearance of the melted mass as taken out of the furnace, and its composition, we shall describe, with some detail, the analytical processes which we have adopted to determine the various elements which exist in pig and wrought iron, and in the samples taken during the puddling.

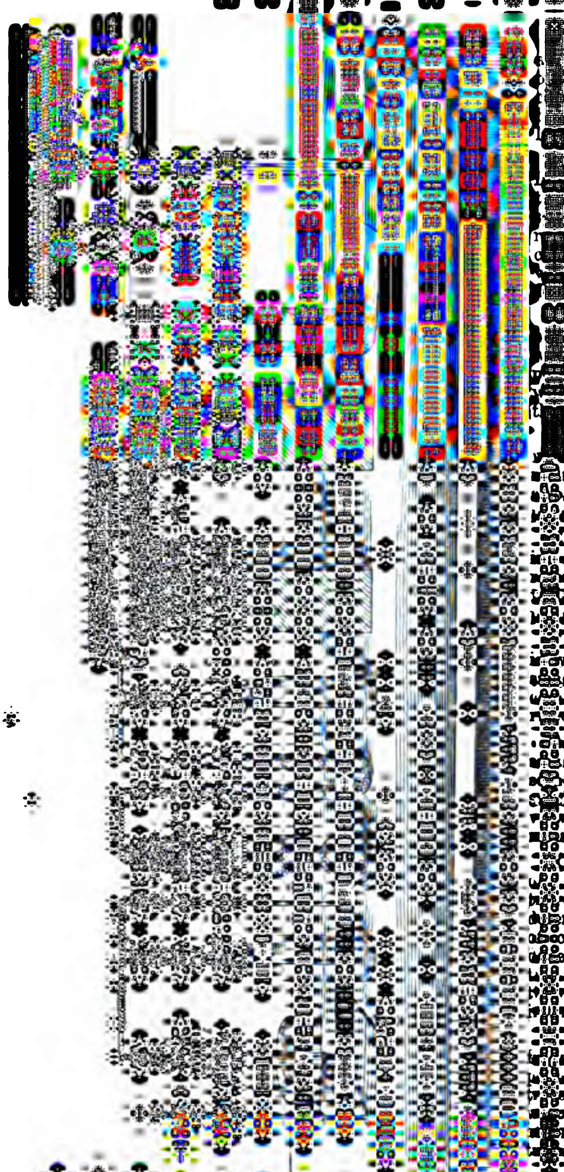
These details of analysis appear to us the more important when it is remembered that most of the heterogeneous substances existing in pig iron, are present only in minute quantities, and that it is on their gradual removal or decrease that the subsequent quality of the wrought iron depends. Also, it is necessary to bear in mind, that we had to trust entirely to the exactitude of the analytical methods adopted to appreciate the chemical changes which gradually took place in the melted mass during the hour and a quarter that the conversion of the cast into wrought iron lasted.

Iron.

The quantity of iron was determined by dissolving one gramme of iron in pure hydrochloric acid, reducing the solution to a perfect protosalt by a little pure zinc, and then determining the amount of iron by Margueritte's process.

Carbon.

To determine this element we found, after many trials, that the best process was to reduce the iron into very fine powder, either by pulverization or by means of a file, and then to burn the carbon under the influence of a red heat by a slow current of pure and dry oxygen gas. The apparatus which we used was the following:—



A flask containing ...xture of chlorate ...tash and oxide of ...er, which by gently ...ng gave off a regu- ...urrent of oxygen.

A bottle contain- ...concentrated solu- ...of caustic potash, ...o retain any chlo- ... or any oxygenated ...ound of this gas, ...h might be pro- ...d.

A tube full of ...cestone moistened ...solution of caustic ...h, and employed ...the same view as ...ast.

A U tube filled ... pieces of solid ...ted caustic potash, ... used for the same ...pose.

A bottle contain- ... sulphuric acid, for ...ning any moisture ...h might accom- ... the oxygen gas.

A porcelain tube ... which was placed a ...l porcelain dish ...aining the pulve- ...d cast iron.

A tube filled with ...ll pieces of pumice- ... moistened with ...huric acid, with the ... of retaining any ...ture.

A Liebig tube full ...concentrated solu- ...of caustic potash, to ...rmine the amount ...arbonic acid pro- ...d by the combina- ...of the oxygen with ...arbon of the cast

A small tube with ...ents of caustic ...h, to retain any ... of carbonic acid ...h might not be ...bed in the Liebig

...cid complete, ...regularly and

slowly; therefore, about two hours are required to burn all the carbon existing in about three grammes of cast iron. By this method, two analyses of the same sample seldom presented a greater difference than 0.005. We also always took the precaution to dissolve the oxide of iron obtained, after combustion, in order to see that no hydrogen was given off and consequently no metallic iron remained.

Silicium.

There is considerable difficulty in determining with precision this element in pig iron, and it was only after several fruitless trials of various processes that we adopted the following, which gave us very satisfactory and concordant results.

Five grammes of pig iron were dissolved in aqua regia containing excess of nitric acid, the whole was then evaporated to dryness and fused in a platinum crucible with three times its weight of a mixture of pure carbonate of potash and carbonate of soda. The mass obtained was dissolved in water and boiled with aqua regia until the whole of the peroxide of iron had entered into solution, and then was evaporated a second time to dryness and heated carefully to about 200° C. The mass was then heated with hydrochloric acid and water, and the silica, on being gathered on a filter, was washed with dilute hydrochloric acid until it was perfectly white; it was then dried, and calcined, and its weight gave the amount of silicium in the iron analysed.

Sulphur.

In consequence of the small proportion in which this element exists in pig and wrought iron, considerable difficulty is experienced in ascertaining with accuracy the various proportions of sulphur existing in iron; and this difficulty was increased by the fact that none of the methods recommended gave satisfactory results in our hands. Thus, for example, the method which consists in determining the sul-

phur in the state of sulphuretted hydrogen failed, owing to the difficulty of removing the last traces of sulphuretted hydrogen held in solution in the liquid in which the iron is dissolved and in which the gas is produced. As to the process which consists in dissolving the iron in aqua regia and boiling off the greatest part of the acid and then adding nitrate of baryta to the solution, it cannot be followed with security, for one of us has shown that sulphate of baryta is soluble in acid liquors, especially in those containing nitric acid, and often in such quantities as to make a greater difference between the analyses of two samples of the same iron than the real difference which exists in two samples of iron from different ores.

These considerations induced us to modify the last mentioned process in the following way. Five grammes of the sample of iron to be analysed were reduced to fine powder and gradually and slowly added to a strongly oxydizing aqua regia, composed of four parts of fuming nitric acid and one part of hydrochloric acid. The iron being dissolved, the solution was evaporated to the consistence of a thin syrup, and then gradually mixed with four times its weight of a mixture of pure carbonates of potash and soda, and heated to redness for one hour in a platinum crucible. The fused mass was then heated with boiling water until all the soluble portion was dissolved. This liquor was then rendered slightly acid with hydrochloric acid, evaporated to dryness, and heated at 200° C., to render the silica insoluble. The whole was then heated by water slightly acidulated with acetic acid, and on the silica being separated by filtration, the amount of sulphate, and consequently, of sulphur, was determined, from the weight of sulphate of baryta obtained.

Phosphorus.

We also attached great importance to the exact valuation of this body, because, like sulphur, its presence even in small

quantities is most injurious, the more so as such minute quantities as a few thousandths more or less will completely alter the value of this metal for many uses. To determine phosphorus, the process which we followed was similar to that employed for the sulphur, with this difference, that we added to the liquid from which we had separated the silica, a little hydrochloric acid, and then ammonia in excess, instead of acetic acid, as we had done in the analysis for sulphur. The liquid was allowed to stand to see if any alumina separated, and if not, we added hydrochloric acid in excess, then pure chloride of calcium and then ammonia again, when phosphate of lime, having the following formula $PO_5 3CaO$, was precipitated, from which the quantity of phosphorus was calculated. We always took care to operate on such a bulk of fluid as to prevent the precipitation of any sulphate of lime, and we also washed rapidly to prevent any carbonate of lime being formed. We verified this method several times during our analyses, by determining the amount of lime in our precipitates and the amount of phosphoric acid by M. Reynoso's process.

Aluminum.

If there was any alumina it was separated during the last process, and its amount determined.

We also made several fusions of iron dissolved in aqua regia, and evaporated with a mixture of alkaline carbonates to which we had added a little caustic alkali and we found no alumina or mere traces in the iron analysed by us.

Manganese.

Five grammes of iron were dissolved in aqua regia, and the whole evaporated to dryness and calcined with alkaline carbonates. The fused mass was treated with boiling water, and to the solution were added small pieces of Swedish paper, to reduce the manganese.

The iron and manganese were then collected on a filter, well washed, and then dissolved in hydrochloric acid. This solution was again evaporated, and heated so as to render the silicic acid insoluble.

The residue was then heated with weak hydrochloric acid, and the solution filtered to separate the silica; carbonate of baryta, recently prepared, was then added to precipitate the oxide of iron; this was separated by filtration, sulphate of soda and a little hydrochloric acid were added to the liquid, to separate the baryta in solution, and finally the manganese was precipitated by a little caustic potash, washed, dried, calcined, and its amount ascertained.

It is necessary that we should describe, in a rapid manner, the physical conditions which pig iron assumes during its conversion. When first melted in a puddling furnace it forms a thick pasty mass, which gradually becomes thin and as fluid as mercury. When it has reached this point it experiences a violent agitation, technically called "the boil," which is produced, no doubt, by the oxidation of the carbon and the escape of the carbonic oxide then generated. During this period of the operation the mass swells to several times its primitive bulk, and the puddler quickly agitates the melted mass to facilitate the oxidation of the carbon. After a short time the mass gradually subsides, the puddler then changes his tool and takes the "puddle" to gather with it the granules of malleable iron floating in the melted mass of scoria or slag. The granules or globules of iron gradually weld together and separate from the scoria, and this separation is hastened by the puddler gradually forming large masses, called balls, weighing from 70 to 80 lbs., from which the scoria drains out.

This part of the operation requires great skill, as nearly the whole of the carbon has been oxidized, so that if the

current of air is not managed with great care the iron itself is oxidized, or, as it is technically termed, "burnt;" and thus not only does great loss ensue in the quantity of malleable iron prod■ed, but also the iron containing a certain quantity of oxide of iron is brittle and of bad quality.

We shall now examine the various chemical changes which pig iron undergoes during its conversion into wrought iron.

The iron we took for our experiments was a good cold blast Staffordshire iron, the pig was rather grey, being of the quality used for making iron wire, or a grey No. 3. Its composition was as follows:—

	First Analysis.	Second Analysis.	Mean.
Carbon	2.320	2.230	2 275
Silicium........................	2.770	2.670	2.720
Phosphorus	0.580	0 710	0.645
Sulphur	0.318	0.288	0.301
Manganese and Aluminum...	Traces	...	...
Iron...	94.059	94.059	94.059
	100.047	99.957	100.000

First Sample, taken out of the furnace at 12 40 p.m.

Two hundred and twenty-four pounds of the above pig iron were introduced at twelve o'clock, on the 14th of April, 1856, into a puddling furnace, which had been cleaned out with wrought iron scraps. After 30 minutes the pigs began to soften and to be easily crumbled, and 10 minutes more had hardly elapsed when they entered into fusion. A sample was immediately taken out from the centre of the melted mass, with a large iron ladle, and poured on a stone flag to cool. The flue of the furnace, which had hitherto been kept open, was now nearly closed by a damper at the top of the chimney, so that the products of combustion came out by the door of the furnace and other openings, whilst little or none escaped by the chimney.

APPEARANCE OF THE SAMPLE.—On breaking the sample as taken out of the furnace, it had no longer the appearance of grey No. 3 pig iron, but had a white, silvery, metallic fracture, similar to that of refined metal. The rapid cooling of the sample was, no doubt, the cause of the change noticed, for it contained quite as much carbon as the pig iron used; and, further, the carbon was in a very similar condition, as in both cases a large quantity of black flakes of carbon floated in the acid liquors in which the iron was dissolved. The following is the amount of carbon and silicium which the above sample contained per cent.

	First Analysis.	Second Analysis.	Mean.
Carbon	2.673	2.780	2.726
Silicium	0.893	0.938	0.915

These results are highly interesting, as they show that the iron had undergone, during the 40 minutes* which it had been in the furnace, two opposite chemical changes; for, whilst the proportion of carbon had increased, the quantity of silicium had rapidly decreased.

This curious fact is still further brought out by the sample which we took out of the furnace at 1 p.m., or 20 minutes later than the last sample analysed, as is shown in this table.

	Carbon.	Silicium.
Pig Iron used	2.275	2.720
First Sample taken out at 12.40	2.726	0.915
Second Sample taken out at 1 p.m.	2.905	0.197

Therefore the carbon had increased 0.630 or 21.5 of its own weight, and the silicium had decreased in the enormous proportion of above 90 per cent. It is probable that these opposite chemical actions are due, in the case of the carbon, to the excess of this element in a state of minute division, or in a nascent state, in the furnace, and that under the influence of the high temperature it combines with the iron, for

which it has a great affinity; whilst the silicium and a small portion of iron are oxidized and combined together to form protosilicate of iron, of which the scoria or slag produced during this first stage of puddling consists, and which plays such an important part in the remaining phenomena of the puddling process.

SECOND SAMPLE, taken out at 1 p.m.

This sample contained the following quantities of carbon and silicium.

	First Analysis.	Second Analysis.	Mean.
Carbon	2.910	2.900	2.905
Silicium	0.226	0.168	0.197

It had the same white silvery appearance as No. 1, but with this difference, that it was slightly malleable under the hammer, instead of being brittle like No. 1. The scoria also is on the upper surface of the mass when cold, and not mixed with the metallic iron as in preceding samples.

THIRD SAMPLE, taken out of the furnace at 1 5 p.m.

The mass in the furnace having become very fluid and beginning to swell or enter into the state called "the boil," a small quantity was ladled out. The appearance of the sample when cold was quite different from that of the two previous ones, being composed of small globules adhering to each other and mixed with the scoria; the mass therefore was not compact like the former ones, but was light and spongy, its external appearance was black, and the small globules, when broken, presented a bright metallic lustre, and were very brittle under the hammer.

We had, for some time, considerable difficulty in separating the scoria from the globules of iron, but we found that by pulverizing the whole for a long time, the scoria was reduced to impalpable powder, and by sieving we could separate it

from the iron, which was much less friable. The iron thus cleansed from its scoria gave us the following results:—

	First Analysis.	Second Analysis.	Mean.
Carbon	2.466	2.421	2.444
Silicium	0.188	0.200	0.194

FOURTH SAMPLE, taken at 1 20 p.m.

As soon as the last sample had been taken out the damper of the furnace was slightly raised, so as to admit a gentle current of air, which did away with the smoke which had been issuing from the puddler's door, and a clear and bright flame was the result. This was done, no doubt, to facilitate the oxidation of the carbon of the iron, and to increase this action the puddler quickly agitated the mass. Under these two actions the mass swelled up rapidly, and increased to at least four or five times its original bulk, and at 1 20 p.m., the mass being in full boil, this fourth sample was taken out. Whilst cooling, it presented the interesting fact, that in various parts of it small blue flames of oxide of carbon were perceived, no doubt arising from the combustion of carbon by the oxygen of the atmosphere.

It is curious that this phenomenon was not observed in the previous samples. It is due probably to the following causes: Firstly, that the pig iron having been brought by the boil to a state of minute division, offers a large surface to the action of the oxygen of the air, and thus the combination of the oxygen with the carbon of the iron is facilitated. And secondly, that at this period the carbon seems to possess little or no affinity for the iron, for one of us has often observed, that when pig iron rich in graphite is puddled, the carbon is liberated from the iron; for if a cold iron rod is plunged into the mass of melted iron in the puddling furnace, it is covered with iron and abundant shining scales of graphite carbon.

The appearance of this, No. 4 sample, was very interesting; and the best idea that we can give of it is, that it is

so light and formed of such minute granules, as to be exactly like an ant's nest. The particles have no adherence to each other, for by mere handling of the mass it falls into pieces. This is due to each particle of iron being intimately mixed with scoria.

The granules of iron have a black appearance externally, are very brittle under the hammer, and when broken present a bright silvery metallic fracture.

The scoria was separated by the method above described for No. 3, and the quantities of carbon and silicium which the iron contained were as follows:—

	First Analysis.	Second Analysis.	Mean.
Carbon	2.335	2.276	2.305
Silicium	0.187	0.178	0.182

FIFTH SAMPLE, taken at 1 35 p.m.

This sample is a most important one in the series, as it is the first in which the iron is malleable and flattens when hammered. It was ladled out of the furnace just as the boil was completed and the swollen mass began to subside. The damper at the top of the chimney was drawn up, so that a very rapid draught was established through the furnace. The puddler also changed his tool, leaving the rubble and taking the puddle to work with.

When cold it partakes of the appearance of Nos. 3 and 4 samples, the mass being spongy and brittle as in No. 4, but less granulated, and like No. 3 being in separate globules, mixed with the scoria. The granules are black externally, but are bright and metallic when flattened. The analysis of these globules proves that the mass of iron in the furnace has lost, during the quarter of an hour which has elapsed since the taking of No. 4 sample, a large proportion of its carbon equal to 20 per cent. of its weight, whilst the silicium on the contrary has remained nearly stationary.

	First Analysis.	Second Analysis.	Mean.
Carbon	1.614	1.681	1.647
Silicium	0.188	0.178	0.186

SIXTH SAMPLE, taken at 1 40 p.m.

The reason why this sample was taken only five minutes after the last sample, was that the mass in the furnace was rapidly transforming itself into two distinct products, viz., the scoria on the one hand and small globules of malleable iron on the other. We attached some importance to this sample, as the workman was on the point of beginning the balling or agglommerating the globules of iron, so as to form large balls of about 80 lbs. weight to be hammered and rolled out into bars. Whilst the mass taken out for analysis was cooling, small blue flames of oxide of carbon issued from it. These were similar to those observed in Nos. 4 and 5, but were not so abundant.

The appearance of this sample was similar to the last one, with the exception that the scoria was not so intimately mixed with the globules of iron, and that these were larger and slightly welded together when hammered. The proportions of carbon and silicium were as follows :—

	First Analysis.	Second Analysis.	Mean.
Carbon	1.253	1.160	1.206
Silicium	0.167	0.160	0.163

When these figures are compared with those of the previous analysis, it is interesting to observe that whilst the silicium remains nearly stationary the carbon rapidly diminishes, for in the five minutes which elapsed between the taking out of the two samples, there are 28 per cent. of the carbon burnt out. This rapid decrease of carbon in the iron is maintained during the remaining ten minutes of puddling. In fact, in one quarter of an hour, viz., from 1 35 to 1 50 the iron lost 50 per cent. of the carbon.

SEVENTH SAMPLE, taken at 1 45 p.m.

This sample was obtained when the puddler had begun to ball. The appearance of the sample, although similar to the

last, differs from it by the granules being rather larger and nearly separated from the scoria, which forms a layer at the top and bottom of the mass.

These granules are also much more malleable, for they are easily flattened under the hammer. This last fact is easily accounted for by the small amount of carbon which it contains, as stated above and shewn by these results.

	First Analysis.	Second Analysis.	Mean.
Carbon	1.000	0.927	0.963
Silicium	0.160	0.167	0.163

EIGHTH SAMPLE, taken 1 50 p.m.

This last sample was taken a few minutes before the balls were ready to be taken out of the furnace to be placed under the hammer, and was a part of one of the balls which was separated and placed to cool.

It was observed that no blue flame issued from the mass as it cooled. The appearance of the sample shewed that the mass constituting the ball was still spongy and granulated, similar to the previous ones. The only difference was that the granules adhered together sufficiently to require a certain amount of force to separate one from the other, and also that they were much more malleable under the hammer. They were found to contain the following quantities of carbon and silicium, per cent.:—

	First Analysis.	Second Analysis.	Mean.
Carbon	0.771	0.773	0.772
Silicium	0.170	0.167	0.168

We should observe here that the black coating which covers the granules of iron, even of No. 8 sample, preserves the iron from all oxidation, for none of the samples became oxidized during the nine months they were in the laboratory exposed to the atmosphere, and to the various acid fumes floating about. This black coating is probably composed of a saline oxide of iron.

Ninth Sample of Puddled Bar.

The balls taken out of the furnace were hammered and then rolled into bars, and in these we found the following:—

	First Analysis.	Second Analysis.	Mean.
Carbon	0.291	0.301	0.300
Silicium	0.130	0.110	0.120
Sulphur	0.142	0.126	0.134
Phosphorus	0.139	...	0.139

Tenth Sample—Wire Iron.

The puddled bars were cut into billets of about four feet in length, and heated in a furnace to white heat, and then rolled into wire iron. The proportions of carbon, silicium, sulphur, and phosphorus, were as follows:—

	First Analysis.	Second Analysis.	Mean.
Carbon	0.100	0.122	0.111
Silicium	0.095	0.082	0.088
Sulphur	0.093	0.096	0.094
Phosphorus	0.117	...	0.117

To complete the series of products in the conversion of pig iron into wrought iron, we analysed the scoria or slag which remained in the furnace after the balls had been taken out, and found its composition to be as follows:—

Silica	16.53
Protoxide of Iron	66.23
Sulphuret of Iron	6.80
Phosphoric Acid	3.80
Protoxide of Manganese	4.90
Alumina	1.04
Lime	0.70
	100.00

Therefore, in the scoria are found the silicium, phosphorus, sulphur, and manganese, which existed in the pig iron; and probably the phosphorus and silicium are removed from the iron by their forming fusible compounds with its oxide.

We shall conclude this paper by giving our results in a tabulated form, so that the removal of the carbon and silicium

may be better appreciated by those who may consult it, with the view of obtaining such information as may lead them to those improvements to which we think our investigations tend.

	TIME.	CARBON.	SILICIUM.
Pig Iron used	12.0.	2.275	2.720
Sample No. 1	12.40 P.M.	2.726	0.915
" 2	1.0. "	2.905	0.197
" 3	1.5. "	2.444	0.194
" 4	1.20 "	2.305	0.182
" 5	1.35 "	1.647	0.183
" 6	1.40 "	1.206	0.163
" 7	1.45 "	0.963	0.163
" 8	1.50 "	0.772	0.168
Puddled Bar 9		0.300	0.120
Wire Iron 10		0.111	0.088

Finally, we wish to express to Mr. Siméon Stoikowitsch our best thanks for the ability and perseverance which he has shown in helping us in these long and tedious analyses.

IX.—*On the 7-partitions of* X.

By the Rev. Thomas P. Kirkman, A.M., Rector of Croft-with-Southworth, and Honorary Member of the Literary and Philosophical Society of Manchester.

[*Read April 7th*, 1857.]

The subject of partitions of numbers has been greatly advanced within the last two years. Euler was, as I believe, the first to show that the number of ways in which x can be made up of $a, b, c \ldots m$, each element being repeated an indefinite number of times, is the co-efficient of r^x in

$$\frac{1}{(1-r^a)(1-r^b)\ldots(1-r^m)};$$

and thus the problem was reduced to the decomposition of rational fractions. This view of the matter, however, led to no practical results, until, by the profound researches of Mr. Cayley, printed in the current volume of the *Philosophical Transactions*, and by the more decisive discoveries of Professor Sylvester, who has, if I mistake not, thoroughly worked out the idea of Euler, and given the results of it in a calculable form, the problem of partitions has been directly solved, without any aid from induction. That is, Professor Sylvester has given the general formula both for the non-circulating and the circulating part of the expression of the k-partitions of X, in the *Quarterly Mathematical Journal* for July, 1855.

The direct solution is obtained by an analysis of no ordinary difficulty; and I think it not unlikely that, for practical use, the inductive method given by me in the twelfth volume

of the Manchester Memoirs will be found most easily applicable.

It will at least be interesting, for a comparison of methods, to show how very easily the 7-partitions of X can be obtained by my method, which requires no expansions, transformations, or summations of any kind, nor anything more than very simple eliminations. Nor has the formula for the 7-partitions, so far as I know, been explicitly found by any one.

That for the 6-partitions was first given by me at p. 143 of the twelfth volume of the Manchester Memoirs, as follows:

$$
\begin{aligned}
(\mathrm{P}) \qquad {}_6\mathrm{P}_x = \frac{1}{60^2.12^2}\Big\{ & 6x^5+135x^4+760x^3-5046x \\
& -(1350x^2+5400x)\,{}^*2_{x-1} \\
& +7926x\cdot{}^*6_x-8424x({}^*6_{x-1}+{}^*6_{x-5}) \\
& +1176x\cdot{}^*6_{x-3}-1674x({}^*6_{x-2}+{}^*6_{x-4}) \\
& +{}^*6_x\,(-47556k \qquad\qquad +180(d_5+d_{11}+d_{17}+\ .\ .\ \text{to } k \text{ terms})) \\
& +{}^*6_{x-1}(+50544k-31225+180(d_0+d_6+d_{12}+\ .\ .\ \text{to } k \text{ terms})) \\
& +{}^*6_{x-2}(+10044k-\ \ 6656+180(d_1+d_7+d_{13}+\ .\ .\ \text{to } k \text{ terms})) \\
& +{}^*6_{x-3}(-\ \ 7056k-\ \ 4617+180(d_2+d_8+d_{14}+\ .\ .\ \text{to } k \text{ terms})) \\
& +{}^*6_{x-4}(+10044k-\ \ 9328+180(d_3+d_9+d_{15}+\ .\ .\ \text{to } k \text{ terms})) \\
& +{}^*6_{x-5}(+50544k-16889+180(d_4+d_{10}+d_{16}+\ .\ .\ \text{to } k \text{ terms}))\Big\};
\end{aligned}
$$

where $x=60h+6(k-1)+c$; $(c>0, \angle 7)$;

$$=60h+p+1; \qquad (k \geqq 0, \angle 10);$$

and $\frac{d_p}{2880}$ is the co-efficient of the circulator ${}^*60_{x-p}$ in ${}_6\mathrm{P}_x$.

In the expression given at p. 143 above quoted, the constant ${}^*2_{x-1}\mathrm{C}'+\mathrm{F}$ is by inadvertence omitted, which stands at the top of p. 142; and at line 13, p. 143, there is a misprint of -3598 for -3528. The omission of the constant ${}^*2_{x-1}\mathrm{C}'+\mathrm{F}$ makes no difference in the value of ${}_6\mathrm{P}_x$, because that value is always the integer nearest, whether over or under, to the sum of the terms in x.

The 60 numbers d_p, are given at p. 139 of the Memoirs quoted above. It is not necessary for our purpose here to

refer to them, except for the remark, which inspection of that page will verify, that

$$d_0+d_1+d_2+d_3+d_4+d_5=91,$$
$$d_{30}+d_{31}+d_{32}+d_{33}+d_{34}+d_{35}=91,$$

the sums of the first and sixth horizontal lines; and that the sum of the six numbers in every other horizontal line at p. 139, beginning with d_6, d_{12}, d_{18}, d_{24}, d_{36}, d_{42}, d_{48}, and d_{54}, is always $=-485$. These horizontal lines appear as vertical lines in the formula for ${}_6\mathrm{P}_x$ just written.

The constant circulator, or term free from x, is obtained from the above formula by giving to k its 10 successive values, 0, 1, 2 . . . 9. As both the sum of the 60 elements of that term and the elements themselves are required, the first for the determination of the terms in x of ${}_7\mathrm{P}_x$, and the latter for that of the circulating constant in it, we must here obtain these data, before attempting to find ${}_7\mathrm{P}_x$.

Adding up the six concluding lines of the formula above written, we obtain

$$\frac{1}{720^2}\left\{66564k-68715k^\circ+180(s_0+s_6+s_{12}+\ .\ .\ .\ \text{to } k \text{ terms})\right\};$$

where $s_p=d_p+d_{p+1}+\ .\ .\ .$ to 6 terms,

$$s_0=s_{30}=91, \text{ and}$$

s_6, s_{12}, s_{18}, s_{24}, s_{36}, s_{42}, s_{48}, are each equal -485.

Giving to k all its values, we have the sum

$$\frac{1}{720^2}\left\{66564\cdot45-68715\cdot10+180\begin{pmatrix}9s_0+8s_6+7s_{12}+6s_{18}+5s_{24}\\+4s_{30}+3s_{36}+2s_{42}+s_{48}\end{pmatrix}\right\}$$

$$=\frac{1}{720^2}\left\{299538-687150+180(13\cdot91-32\cdot485)\right\},$$

$$=-\frac{272430}{720^2}=\frac{1}{720^2}(f_0+f_1+f_2+\ .\ .\ .\ +f_{59}),$$

the sum of the 60 elements of the circulating constant, whose form is $\Sigma f_p{*}60_{x-p}\cdot\frac{1}{720^2}$.

If it could be proved true for ${}_7\mathrm{P}_x$, as it is for all the lower partitions of x, that the sum of the terms in x expressed the number of the partitions, this sum just found, together with

the terms in x of ${}_6P_x$, would easily give us ${}_7P_x$; for they give us, as we shall see, very readily all the terms in x of ${}_7P_x$. But as this does not appear certainly true, it becomes necessary to write out in full the constant term of ${}_6P_x$, that we have it in our power accurately to express the constant of ${}_7P_x$, which, unlike this of ${}_6P_x$, we may find essential to the value.

This constant is, writing the numerators f_1, f_8, &c.,

$f_1=\ 19319$	$f_2=-\ 5008$	$f_3=\ 7047$	$f_4=-62464$
$f_8=-48128$	$f_9=-\ 46089$	$f_{10}=-50800$	$f_{11}=\ 45319$
$f_{15}=-34425$	$f_{16}=\ -256$	$f_{17}=-\ 7817$	$f_{18}=\ 73872$
$f_{22}=\ 11408$	$f_{23}=\ 3847$	$f_{24}=\ 20736$	$f_{25}=-63625$
$f_{29}=-49289$	$f_{30}=\ 32400$	$f_{31}=\ 51719$	$f_{32}=-27392$
$f_{36}=\ 82944$	$f_{37}=\ -1417$	$f_{38}=-15728$	$f_{39}=-13689$
$f_{43}=\ 10247$	$f_{44}=-\ 68864$	$f_{45}=-66825$	$f_{46}=\ 32144$
$f_{50}=-57200$	$f_{51}=\ 48519$	$f_{52}=-20992$	$f_{53}=-28553$
$f_{57}=-\ 4617$	$f_{58}=\ -9328$	$f_{59}=-16889$	$f_{60}=\ 0$

$f_5=-70025$	$f_6=\ 115344$	$f_7=\ 30983$
$f_{12}=\ 62208$	$f_{13}=-\ 22153$	$f_{14}=-36464$
$f_{19}=-10489$	$f_{20}=-\ 89600$	$f_{21}=\ 16119$
$f_{26}=\ 25744$	$f_{27}=\ 27783$	$f_{28}=-41728$
$f_{33}=-25353$	$f_{34}=-\ 30064$	$f_{35}=-37625$
$f_{40}=-83200$	$f_{41}=\ 12919$	$f_{42}=\ 94608$
$f_{47}=\ 24583$	$f_{48}=\ 41472$	$f_{49}=-42889$
$f_{54}=\ 53136$	$f_{55}=-\ 31225$	$f_{56}=-\ 6656$

These numbers can all be obtained from $f_{60}=0$, $f_{59}=-16889$, $f_{58}=-9328$, $f_{57}=-4617$, $f_{56}=-6656$, and $f_{55}=-31225$, by the formula

$$\begin{aligned} f_{p+6}-f_p= & \ 11664(12_{x-2}+12_{x-3}+12_{x-6}+12_{x-7}+12_{x-10}+12_{x-11}) \\ & \times(1-20_{x-6}-20_{x-11}) \\ & -53136(12_x+12_{x-1}+12_{x-4}+12_{x-5}+12_{x-8}+12_{x-9}) \\ & \times(1-20_{x-1}-20_{x-16}) \\ & +115344.(20_{x-6}+20_{x-11})+50544.(20_{x-1}+20_{x-16}). \end{aligned}$$

And it is remarkable, that

$$f_{p+30}-f_p=\pm 180^2.$$

The reason of all this may be found in the fact, that the elements d_p of the circulating constant in ${}_6P_x$ all satisfy, except only twelve of them,

$$d_{p+1}=d_{p-1};$$

while all of them satisfy

$$d_{p+30}-d_p=\pm 360.$$

We proceed to deduce from the expression for ${}_6P_x$, that for ${}_7P_x$, the 7-partitions of x.

I have suffered no little perplexity in the endeavour to obtain ${}_7P_x$ from the consideration of the equation

$${}_kP_x-{}_kP_{x-k}={}_{k-1}P_{x-1},$$

which gave me readily my previous results. But all difficulty disappears if we use the two equations

$${}_7P_x-{}_7P_{x-7}={}_6P_{x-1}, \text{ and}$$

$${}_7P_x-{}_7P_{x-42}={}_6P_{x-1}+{}_6P_{x-8}+\,.\,.+{}_6P_{x-36},$$

which follow easily from the fundamental theorem

$${}_kP_x={}_{k-1}P_{x-1}+{}_{k-1}P_{x-k-1}+{}_{k-1}P_{x-2k-1}+\,.\,.\,.,\text{ or,}$$

writing $x=7(e-1)+c$, $(c>0, \angle 8)$,

$${}_7P_x={}_6P_{x-1}+{}_6P_{x-8}+{}_6P_{x-15}+\,.\,.\,.+{}_6P_{c-1}. \quad \text{(A.)}$$

Let $({}_7P_x)$ stand for the sum of the algebraic, or non-circulating terms in the right member of equation (A), and $({}_7P_{x-7})$ for the sum of such terms in

$${}_7P_{x-7}={}_6P_{x-8}+{}_6P_{x-15}+\,.\,.\,.+{}_6P_{c-1}. \quad \text{(A}')$$

It is plain that, if $[{}_6P_{x-1}]$ represent the non-circulating part of ${}_6P_{x-1}$, we shall have

$$({}_7P_x)-({}_7P_{x-7})=[{}_6P_{x-1}]. \qquad \text{(B)}$$

Let then

$$({}_7P_x)=Ax^6+Bx^5+Cx^4+Dx^3+Ex^2+Fx+Gx;$$

for evidently no power above the 6th can disappear by the above subtraction. Then (B) is

$$A\left(x^6-(x-7)^6\right)+B\left(x^5-(x-7)^5\right)+C\left(x^4-(x-7)^4\right)+D\left(x^3-(x-7)^3\right)$$
$$+E\left(x^2-(x-7)^2\right)+F\left(x-(x-7)\right)$$
$$=\frac{1}{(720)^2}\left\{6(x-1)^5+135(x-1)^4+760(x-1)^3-5046(x-1)\right\}$$

In this subtraction the constant G_x must vanish of course, if it be an ordinary constant; and if it be a circulator, it can depend only on the 7 values of c in equation (A); that is, it must be of the form

$$G_1 \cdot {}^*7_{x-1} + G_2 \cdot {}^*7_{x-2} + \quad . \quad . \quad . \quad + G_0 \cdot {}^*7_x,$$

so that $G_x = G_{x-7}$, and can have only seven values.

Equating the co-efficients of powers of x, we have

$$42A = \frac{6}{720^2}, \therefore A = \frac{1}{7 \cdot 720^2};$$

$$-15 \cdot 7^2 A + 35B = \frac{105}{720^2}, \therefore B = \frac{6}{720^2};$$

$$20 \cdot 7^3 A - 10 \cdot 7^2 B + 4 \cdot 7C = \frac{280}{720^2}, \therefore C = \frac{80}{720^2};$$

$$-15 \cdot 7^4 A + 10 \cdot 7^3 B - 6 \cdot 7^2 C + 3 \cdot 7D = -\frac{1530}{720^2}, \therefore D = \frac{2185}{7 \cdot 720^2};$$

$$6 \cdot 7^5 A - 5 \cdot 7^4 B + 4 \cdot 7^3 C - 3 \cdot 7^2 D + 27E = -\frac{3276}{720^2}, \therefore E = -\frac{1361}{2 \cdot 720^2};$$

$$-7^6 A + 7^5 B - 7^4 C + 7^3 D - 7^2 E + 7F = \frac{4415}{720^2}, \therefore F = -\frac{55899}{14 \cdot 720^2}.$$

Wherefore

$$({}_7P_x) = \frac{x^6}{7 \cdot 720^2} + \frac{6x^5}{720^2} + \frac{80x^4}{720^2} + \frac{2185x^3}{7 \cdot 720^2} - \frac{1361x^2}{2 \cdot 720^2} - \frac{55899x}{14 \cdot 720^2} + G_x.$$

The value of the constant G_x is obtained from the equation, (putting $x = 7(1-1)+c$)

$$({}_7P_c) = ({}_7P_{c-7}) + [{}_6P_{c-1}] = [{}_6P_{c-1}],$$

for ${}_7P_{x-7}$ has no terms at all on the right of equation (A'), if $x \angle 8$; *i.e.*, $({}_7P_{c-7}) = 0$. This constant therefore will be a circulator of the form $\Sigma G_m{}^*.7_{x-m}$.

The value of G_7 is readily found by the equation

$$({}_7P_0) = ({}_7P_{0-7}) + [{}_6P_{0-1}] = 0 = G_7;$$

for $x=0$, which requires $e=0$, $c=7$, makes every term on the right of equation (A) vanish. The remaining six values, or all the seven, are obtained by putting for c its successive values in

$$({}_7P_c) = [{}_6P_{c-1}],$$

which is

$$\frac{c^6}{7.720^2}+\frac{6.c^5}{720^2}+\frac{80c^4}{720^2}+\frac{2185.c^3}{7.720^2}-\frac{1361c^2}{2.720^2}-\frac{55899c}{14.720^2}+G_c$$

$$=\frac{1}{720^2}\left\{6(c-1)^5+135(c-1)^4+760(c-1)^3-5046(c-1)\right\}.$$

Let now $({}_7P_x)'$ stand for the sum of the circulating terms in x on the right of equation A; and $[{}_6P_{x-1}]'$ for the like terms of ${}_6P_{x-1}$. Then evidently

$$({}_7P_x)'-({}_7P_{x-42})'=[{}_6P_{x-1}]'+[{}_6P_{x-8}]'+\ \cdots\ +[{}_6P_{x-36}]'; \quad \text{(C)}$$

for if we put in (A) $x-42$ for x, and subtract the result from (A), the remainder on the right must be the terms here presented. And it is plain that no power of x higher than the third can have disappeared on the right of (C) by the subtraction. Let

$$({}_7P_x)'=ax^3+bx^2+cx+d_x; \quad \text{(c)}$$

then (C) becomes

$$a\left(x^3-(x-42)^3\right)+b\left(x^2-x-42)^2\right)+c\left(x-(x-42)\right) \quad =$$

$$\frac{1}{720^2}\Big\{\left(-1350(x-1)^2-5400(x-1)\right)2_{x-2}+\left(-1350(x-8)^2\right.$$
$$\left.-5400(x-8)\right)2_{x-9}$$
$$+\left(-1350(x-15)^2-5400(x-15)\right)2_{x-16}+\left(-1350(x-22)^2\right.$$
$$\left.-5400(x-22)\right)2_{x-23}$$
$$+\left(-1350(x-29)^2-5400(x-29)\right)2_{x-30}+\left(-1350(x-36)^2\right.$$
$$\left.-5400(x-36)\right)2_{x-37}$$
$$+(x-1)(+7926{\cdot}6_{x-1}-8424{\cdot}6_{x-2}-1674{\cdot}6_{x-3}+1176{\cdot}6_{x-4}$$
$$-1674{\cdot}6_{x-5}-8424{\cdot}6_{x-6})$$
$$+(x-8)(-8424{\cdot}6_{x-13}+7926{\cdot}6_{x-8}-8424{\cdot}6_{x-9}-1674{\cdot}6_{x-10}$$
$$+1176{\cdot}6_{x-11}-1674{\cdot}6_{x-12})$$
$$+(x-15)(-1674{\cdot}6_{x-19}-8424{\cdot}6_{x-20}+7926{\cdot}6_{x-15}-8424{\cdot}6_{x-16}$$
$$-1674{\cdot}6_{x-17}+1176{\cdot}6_{x-18})$$
$$+(x-22)(+1176{\cdot}6_{x-25}-1674{\cdot}6_{x-26}-8424{\cdot}6_{x-27}+7926{\cdot}6_{x-22}$$
$$-8424{\cdot}6_{x-23}-1674{\cdot}6_{x-24})$$
$$+(x-29)(-1674{\cdot}6_{x-31}+1176{\cdot}6_{x-32}-1674{\cdot}6_{x-33}-8424{\cdot}6_{x-34}$$
$$+7926{\cdot}6_{x-29}-8424{\cdot}6_{x-30}$$
$$+(x-36)(-8424{\cdot}6_{x-37}-1674{\cdot}6_{x-38}+1176{\cdot}6_{x-39}-1674{\cdot}6_{x-40}$$
$$-8424{\cdot}6_{x-41}+7926{\cdot}6_{x-36})\Big\}$$

The constant d_x must disappear in this subtraction (C); for if it be a circulator, it can depend only on the value of c in equation (A) combined with the circulators in $[{}_6P_x]'$; that is, it can have only 42 values, and d_x must be the same with d_{x-42}.

The circulators 2_{x-2}, 2_{x-16}, &c., 6_{x-1}, 6_{x-13}, &c., standing vertically under each other, are equal, if $x > 40$; supposing then that $x = 42(l-1)+\gamma$, $(\gamma > 0, < 43)$, and that $l > 1$, we have, by equating co-efficients,

$$3.42a = -\frac{1350}{720^2}.3(2_x+2_{x-1}) = -\frac{3.1350}{720^2} \qquad \therefore a = -\frac{1350}{42.720^2}$$

$$-3.42^2a+84b = -\frac{3.5400}{720^2}+\frac{1350}{720^2}\left\{(2+30+58)2_{x-2} \right.$$
$$\left. +(16+44+72).2_{x-1}\right\}$$

$$+\frac{1}{720^2}(7926-8424-1674+1176-1674-8424).(6_{x-1}+6_{x-2}$$
$$+6_{x-3}+6_{x-4}+6_{x-5}+6_x),$$

$$i.e.\ 84b = -\frac{3.42^2.1350}{42.720^2}+\frac{90.1350}{720^2}+\frac{42.1350}{720^2}.2_{x-1}-\frac{27294}{720^2};$$

$$\therefore b = \frac{-37947+28350.2_{x-1}}{42.720^2}.$$

$$a.42^3-b.42^2+c.42 = -\frac{1350}{720^2}\left((1+15^2+29^2)2_x+(8^2+22^2+36^2)2_{x-1}\right)$$
$$+\frac{5400}{720^2}\left((1+15+29)2_x+(8+22+36)2_{x-1}\right)$$

$$+\frac{6_{x-1}}{720^2}(-7926+8.8424+15.1674-22.1176+29.1674+36.8424)$$

$$+\frac{6_{x-2}}{720^2}(8424-8.7926+15.8424+22.1674-29.1176+36.1674)$$

$$+\frac{6_{x-3}}{720^2}(1674+8.8424-15.7926+22.8424+29.1674-36.1176)$$

$$+\frac{6_{x-4}}{720^2}(-1176+8.1674+15.8424-22.7926+29.8424+36.1674)$$

$$+\frac{6_{x-5}}{720^2}(1674-8.1176+15.1674+22.8424-29.7926+36.8424)$$

$$+\frac{6_x}{720^2}(8424+8.1674-15.1176+22.1674+29.8424-36.7926).$$

Putting $6_x+6_{x-2}+6_{x-4}$ for 2_x, and $6_{x-1}+6_{x-3}+6_{x-}$ for 2_{x-1}, and $1=6_x+6_{x-1}+6_{x-2}+6_{x-3}+6_{x-4}+6_{x-5}$, we readily obtain

$$c=\frac{1}{42\cdot720^2}\left\{-409860\cdot{}^*6_x+255840\cdot{}^*6_{x-1}-275460\cdot{}^*6_{x-2}\right.$$
$$\left.-12960\cdot{}^*6_{x-3}-141060\cdot{}^*6_{x-4}+121440\cdot{}^*6_{x-5}\right\}$$

Therefore

$$({}_7\mathrm{P}_x)'=\frac{1}{42\cdot720^2}\left\{-1350\cdot x^3-(37947-28350\cdot{}^*2_{x-1})x^2\right.$$
$$-{}^*6_x\cdot409860x+{}^*6_{x-1}\cdot255840x-{}^*6_{x-2}\cdot275460x$$
$$\left.-{}^*6_{x-3}\cdot12960x-{}^*6_{x-4}141060x+{}^*6_{x-5}\cdot121440x\right\}+d_x.$$

The value of the constant $d\gamma$ is given by equation (C), thus:

$$({}_7\mathrm{P}\gamma)'=[{}_6\mathrm{P}_{\gamma-1}]'+[{}_6\mathrm{P}_{\gamma-8}]'+\ \cdot\ \cdot\ +[{}_6\mathrm{P}_{\gamma-36}]', \qquad (\mathrm{C}')$$

if $\gamma \overline{\overline{\angle}} 42$; for in that case ${}_7\mathrm{P}_{\gamma-42}$ has no terms at all on the right side of equation (A), x being $\overline{\overline{\angle}}0$. That of d_{42} is obtained immediately by putting $x=0$ in (C) which requires $l=0$, $\gamma=42$; so that (C) becomes, by (c),

$$({}_7\mathrm{P}_0)'=0+d_{42}=0, \text{ or}$$
$$d_{42}(=d_{x-42l})=d_0=0$$

The remaining 41 values of d_γ, or all the 42 values, are obtained by putting for γ its successive values, 1, 2, 3 ... 42, in the equation (C′), which is, (c) and (P),

$$-d\gamma=\frac{1}{42\cdot720^2}\left\{-1350\gamma^3-(37947-28350\cdot2_{\gamma-1})\gamma^2\right.$$
$$-6_\gamma\cdot409860\gamma-6_{\gamma-4}\cdot141060\gamma-6_{\gamma-2}\cdot275460\gamma$$
$$\left.+6_{\gamma-1}\cdot255840\gamma+6_{\gamma-5}121440\gamma-6_{\gamma-3}12960\gamma\right\}$$
$$+\frac{1}{720^2}\left\{1350\begin{pmatrix}(\gamma-1)^2\cdot2_{\gamma-2}+(\gamma-15)^2\cdot2_{\gamma-16}+(\gamma-29)^2\cdot2_{\gamma-30}\\+(\gamma-8)^2\cdot2_{\gamma-9}+(\gamma-22)^2\cdot2_{\gamma-23}+(\gamma-36)^2\cdot2_{\gamma-37}\end{pmatrix}\right.$$
$$+5400\begin{pmatrix}(\gamma-1)2_{\gamma-2}+(\gamma-15)2_{\gamma-16}+(\gamma-29)2_{\gamma-30}\\+(\gamma-8)2_{\gamma-9}+(\gamma-22)2_{\gamma-23}+(\gamma-36)2_{\gamma-37}\end{pmatrix}$$

$$
\begin{aligned}
&+8424\big((\gamma-1)\cdot(6_{\gamma-2}+6_{\gamma-6})+(\gamma-15)(6_{\gamma-16}+6_{\gamma-20})\\
&\qquad\qquad+(\gamma-29)(6_{\gamma-30}+6_{\gamma-34})\big)\\
&-1176\big((\gamma-1)\cdot 6_{\gamma-4}+(\gamma-15)\cdot 6_{\gamma-18}+(\gamma-29)6_{\gamma-32}\big)\\
&+1674\big((\gamma-8)\cdot(6_{\gamma-10}+6_{\gamma-12})+(\gamma-22)(6_{\gamma-24}+6_{\gamma-26})\\
&\qquad\qquad+(\gamma-36)(6_{\gamma-38}+6_{\gamma-40})\big)\\
&-7926\big((\gamma-8)\cdot 6_{\gamma-8}+(\gamma-22).6_{\gamma-22}+(\gamma-36)\,6_{\gamma-36}\big)\\
\\
&-7926\big((\gamma-1)\cdot 6_{\gamma-1}+(\gamma-15)\cdot 6_{\gamma-15}+(\gamma-29)6_{\gamma-29}\big)\\
&+1674\big((\gamma-1)(6_{\gamma-3}+6_{\gamma-5})+(\gamma-15)(6_{\gamma-17}+6_{\gamma-19})\\
&\qquad\qquad+(\gamma-29)(6_{\gamma-31}+6_{\gamma-33})\big)\\
&-1176\big((\gamma-8)\cdot 6_{\gamma-11}+(\gamma-22)\cdot 6_{\gamma-25}+(\gamma-36)\cdot 6_{\gamma-39}\big)\\
&+8424\big((\gamma-8)(6_{\gamma-9}+6_{\gamma-13})+(\gamma-22)(6_{\gamma-23}+6_{\gamma-27})\\
&\qquad\qquad+(\gamma-36)(6_{\gamma-37}+6_{\gamma-41})\big)\Big\}.
\end{aligned}
$$

In this expression it is to be borne in mind that $6_{x-7}=0$, whenever $x\angle 7$.

We have now found, in the sum

$$({}_7P_x)+({}_7P_x)',$$

all the terms in x of ${}_7P_x$ which arise from summing all the powers >0 of $x-1$, $x-8$, &c., on the right of equation (A). We have yet to determine $({}_7P_x)''$, the sum of the constants in the same member of (A). Of these there are 60 in every term ${}_6P_{x-1-7a}$, of the form $\frac{1}{720^2}\times f_g.{}^*60_{x-g}$, of which, for a given value of x, only one has value, viz. one of the sixty already given, p. 140.

We know that

$$({}_7P_x)''-({}_7P_{x-420})''=[{}_6P_{x-1}]''+[{}_6P_{x-8}]''+[{}_6P_{x-15}]''+\,..\,+[{}_6P_{x-414}]''. \quad \text{(D)}$$

If we write $x=420m+60(n-1)+\theta+1$, we have, for $\theta=0$, $x-1=60y+0$, $x-8=60y+53$, $x-15=60y+46$, &c., so that the right member of this equation is $\frac{1}{720^2}\big\{f_0+f_{53}+f_{46}+f_{39}+\ldots$ to 60 terms$\big\}$, that is, it is always the sum of the 60 numbers f_g, the co-efficients of the circulating constant in ${}_6P_x$.

Let now

$$({}_7P_x)''=ax+\beta_x,$$

The equation D is

$$ax-a(x-420)=\frac{1}{720^2}(f_0+f_1+f_2+\ldots+f_{60});$$

for the constant β_x, depending only on the seven values of c and the circulating constant in ${}_6P_x$, can have only 420 values, and $\beta_x=\beta_{x-420}$, which therefore disappears in D. Wherefore

$$420a=-\frac{272430}{720^2},$$

as has been already proved, and

$$a=-\frac{27243}{42.720^2}.$$

The constant β_x is given by putting $x=x'$, $\overline{\angle}420$; which requires

$$({}_7P_{x'})''=[{}_6P_{x'-1}]''+[{}_6P_{x'-8}]''+\ldots+[{}_6P_{c-1}]'',\ i.e.$$

$$ax'+\beta_x=\frac{1}{720^2}(f_{c-1}+f_{c+6}+\ldots \text{ to } f_{x'-1} \text{ inclusive}),$$

$$\beta_x=\frac{1}{42.720^2}\left\{27243x'+42(f_{c-1}+f_{c+6}+\ldots \text{ to } f_{x'-1})\right\};$$

and $({}_7P_x)''=\dfrac{1}{42.720^2}\left\{-27243(x-x)'+42(f_{c-1}+f_{c+6}+\ldots \text{ to } f_{x'-1})\right\}$.

This being added to $({}_7P_x)+({}_7P_x)'$ found before, completes the expression of ${}_7P_x$.

We have then finally, writing

$$\begin{aligned} x&=420m+x' \\ &=420m+42(n-1)+\gamma \\ &=420m+42(n-1)+7(i-1)+c; \end{aligned}$$

$$(i\overline{\overline{>}}0,\ \angle 7),\ (c>0, \angle 8),\ (\gamma>0,\ \angle 43),\ (x'\overline{\overline{>}}0,\ \angle 420),$$

$$\begin{aligned} {}_7P_x=\Big[\frac{1}{14\cdot720^2}\Big\{&2(x^6-c^6)+84(+x^5-c^5)+1120(x^4-c^4)+4370(x^3-c^3) \\ &-9527(x^2-c^2)-55899(x-c)\Big\} \\ -\frac{1}{42\cdot720^2}\Big\{&1350(x^3-\gamma^3)+(37947-28350.{}^*2_{n-1})(x^2-\gamma^2) \\ &+27243(x-x') \end{aligned}$$

$$+(x-\gamma)({}^*6_x\cdot 409860-{}^*6_{x-1}255840+{}^*6_{x-2}275460$$
$$+{}^*6_{x-3}\cdot 12960+{}^*6_{x-4}\cdot 141060-{}^*6_{x-5}\cdot 121440)\Big\}$$

$$+\frac{1}{720^2}\Big\{6(c-1)^5+136(c-1)^4+760(c-1)^3-5046(c-1)$$
$$-1350((\gamma-1)^2+(\gamma-15)^2+(\gamma-29)^2).{}^*2_x$$
$$-1350((\gamma-8)^2+(\gamma-22)^2+(\gamma-36)^2){}^*2_{x-1}$$
$$-5400(\gamma-1+\gamma-15+\gamma-29){}^*2_x$$
$$-5400(\gamma-8+\gamma-22+\gamma-36){}^*2_{x-1},$$

(taking and squaring only as many of these six remainders as are positive,)

$$+\{(c-1)+(c+6)+(c+13)+\ldots \text{to } i \text{ terms}\}$$

multiplied singly in order by the c^{th}, $(c+1)^{th}$, $(c+2)^{th}$ . . . of the co-efficients

$$(7926,-8424,-1674,+1176,-1674,-8424)$$

of which the e^{th} is the $(e+6)^{th}$,

$$+(f_{c-1}+f_{c+6}+f_{c+13}+\ldots \text{to } (6(n-1)+i) \text{ terms})\Big\}\Big],$$

where $f_p:720^2$ is the co-efficient of $P{}^*60_{x-p}$ in ${}_6P_x$, and $f_{p+60}=f_p$.

In the above expression the portion

$$\beta_x=\frac{1}{720^2}\Big\{\frac{272430x'}{420}+\Big(f_{c-1}+f_{c+6}+f_{c+13}+\ldots \text{to } \frac{x'+7-c}{7}\text{terms}\Big)\Big\}$$

may be disregarded. For it is

$$\beta_x=\frac{1}{720^2}\Big\{-\frac{x'}{7}\Big(\frac{f_1+f_2+f_3+\ldots+f_{60}}{60}\Big)+\Big(f_{c-1}+f_{c+6}+\ldots \text{to } \frac{x'+7-c}{7}\text{ terms}\Big)\Big\};$$

that is, it is always the sum of h terms f_{c-1}, f_{c+6}, &c., beginning with one of the 7 $f_0\ f_1\ f_2\ f_3\ f_4\ f_5\ f_6$, diminished by about h times the mean value of the whole 60 numbers f_p. When x' is small, β_x is the sum or difference of two small numbers. When x' is larger, it is seen by inspection of the table of these quantities f_p that no considerable number of them can be taken as $f_{c-1}+f_{c+6}$, &c., that shall not have a negative sum, and shall not with $+\frac{x}{7}\cdot\frac{272430}{60}$ make the numerator of β_x very far less than $259200=\frac{1}{2}720^2$.

This proves that the value of ${}_7\mathrm{P}_x$ is always the integer nearest, above or below, to the sum of the terms in x, c, γ; x', and f_{c-1}, &c., being neglected; and I suspect, from the look of the expression, that c and γ may be neglected also.

The terms free from x in ${}_7\mathrm{P}_x$ can be reduced to a circulating constant of 420 terms. The shape in which I leave it has at least the advantage of occupying less room, and of showing more of the structure of the function. I am content to lay before the reader a definite expression of these 7-partitions of x, and am convinced that ${}_8\mathrm{P}_x$ can be deduced from it with ease by the method here given; and I think this can be done without the labour of writing out at length this tedious constant in ${}_7\mathrm{P}_x$.

The portions $({}_8\mathrm{P}_x)$ and $({}_8\mathrm{P}_x)'$ of ${}_8\mathrm{P}_x$ are very easily obtained from the equations

$$({}_8\mathrm{P}_x)-({}_8\mathrm{P}_{x-8})=[{}_7\mathrm{P}_{x-1}],$$

$$({}_8\mathrm{P}_x)'-({}_8\mathrm{P}_{x-24})'=[{}_7\mathrm{P}_{x-1}]'+[{}_7\mathrm{P}_{x-9}]'+[{}_7\mathrm{P}_{x-16}]',$$

by a process exactly like that employed to find $({}_7\mathrm{P}_x)$ and $({}_7\mathrm{P}_x)'$. The sum $({}_8\mathrm{P}_x)''$ of the circulating constants in ${}_7\mathrm{P}_{x-1}+{}_7\mathrm{P}_{x-9}+\ldots$ is to be found from

$$({}_8\mathrm{P}_x)''-({}_8\mathrm{P}_{x-840})''=[{}_7\mathrm{P}_{x-1}]''+[{}_7\mathrm{P}_{x-9}]''+\ldots+[{}_7\mathrm{P}_{x-833}]'',$$

the right side of which is the sum of 105 out of the 420 elements of the constant in ${}_7\mathrm{P}_x$. Hence the sum $({}_8\mathrm{P}_x)''$ of all these will be of the form

$$\mathcal{S}=a^*4_x+b^*4_{x-1}+c^*4_{x-2}+d^*4_{x-3},$$

and can be found easily before the constant in ${}_7\mathrm{P}_x$ is fully written out. But I hope that a still more expeditious mode of determining $\mathcal{S}$ will be discovered.

In like manner the 9-partitions, 10-partitions, &c. of x may be deduced, each almost by simple transcription from the last preceding, if the constant of this be written out from the formula arising in the process.

X.—*Remarks on the Occultation of Jupiter and his Satellites by the Moon, January 2nd,* 1857.

By the Rev. Henry Halford Jones, F.R.A.S., &c.

[*Read 24th March,* 1857.]

The following observations were made at the Rusholme-road Cemetery, Manchester.

	Greenwich Mean Time.		
	h.	m.	s.
The fourth Satellite disappeared	4	47	36
The third ditto ditto	4	54	38
First contact of Jupiter with the Moon	5	0	58
Total immersion of Jupiter	5	2	41
The first Satellite disappeared	5	3	21
The second ditto ditto	5	4	27
Last contact of Jupiter with the Moon	5	55	50

The time elapsed between the first contact of Jupiter and his total disappearance was 1 minute and 43 seconds. And the time between the beginning and end of the occultation of the planet, 54 minutes and 52 seconds.

In the earlier part of the evening and during the time in which the several occultations took place, the weather was very fine, and all the objects were beautifully distinct. Not only was the disc of the planet well defined, but his belts and satellites were seen without the slightest difficulty. Not long after the last satellite had disappeared, but more particularly about the time of the planet's reappearance, flying clouds began to interpose, and thus rendered the emersion of the satellites invisible, and the last observed contact of Jupiter with the Moon indistinct and doubtful in time to the extent

of several seconds. In a few minutes afterwards the sky became quite obscure, and a heavy shower of rain began to fall.

The observations were made with a seven-inch Newtonian reflector, and all the phenomena watched with as much vigilance as could be commanded on the occasion, but no perceptible distortion of the planet or the limb of the Moon was observed to take place. Nor did there appear to be any greater difference in the colour of the discs of either Jupiter or the Moon than what might naturally have been expected. During the process of the immersion of the planet, there certainly did seem to be a rather darkly shaded line of demarcation between Jupiter and that part of the Moon's limb projected on the planet's disc. The same phenomenon occurred during the emersion of the planet; but I have a strong impression that this was nothing more than an ocular illusion, occasioned by the juxtaposition and contrast of two objects reflecting differently coloured light, and also differing widely in the intensity of their illumination. Astronomically considered, the whole phenomena were extremely interesting, and, viewed with a contemplative eye, presented a scene of surpassing beauty and sublimity, calculated to awaken in the reflective mind feelings of unusual and intense admiration.

XI.—*Some Peculiarities of the Vital Statistics of the Society of Friends.*

By Alfred Fryer.

[*Read April 7th,* 1857.]

It is now universally acknowledged that the amount of suffering and death caused by the breaking of certain well-known sanitary laws, renders the yearly preventible mortality of our towns greater than that of the most bloody campaign; the number of deaths annually in this country, traceable to causes within our control, is five times as great as the total number of killed, wounded, and missing of the allied army at Waterloo,—thus filth and miasma are more terrible than the sword and the bullet. The mortality of the people in the town is 27 per cent. greater than those in the country districts; anything bearing upon this subject becomes therefore important.

The population of towns, and especially manufacturing towns, comprehends so large a proportion of the poorer classes, that any statistics covering the whole, fail to indicate the amount of mortality due to the vitiated atmosphere and other pernicious effects of densely peopled districts, as they include an excess of deaths due to the occupations, habits, and privations of the poor.

It then becomes an important question: Is there any difference in the average duration of life of two portions of a class of individuals, one living in towns and the other in the country, and both provided with sufficient of the necessaries

of life? If there is any difference, how much? And, if so, at what time of life is the poison of towns most fatal?

Little is known of the average duration of life of the same class of people for successive generations.

To endeavour to throw some light on the above questions is the object of this paper; nevertheless several other subjects are included, some presenting features of novelty, others corroborative of facts already known to the statistician.

In comparing the average duration of life in different times and places, it has appeared difficult to obtain any community whose general circumstances did not allow of changes sufficient to disturb the calculation.

The Society of Friends offers itself as the most promising; their habits have changed less than those of any well-marked class, and they have kept the same relative position in the wealth and industry of the country.

It is pretty well known that the average duration of life in the Society of Friends greatly exceeds that of the people of England generally. The fact has been recorded in the first "report of the commissioners for inquiring into the state of large towns and populous districts;" and is also pointed out in the last edition of Johnstone's Physical Atlas, in the letterpress appended to the map of "health and disease."

This low rate of mortality has been usually attributed to the following circumstances:—

1st. Great attention is paid to cleanliness in their persons and houses.

2nd. They rarely drink to excess or indulge in other intemperance.

3rd. Their children receive careful instruction, and the Society takes charge of the education of the poor among them.

4th. None of their members suffer from the want of suitable food or adequate clothing, as the Society maintains its own poor.

5th. The women are not often employed in other than domestic labour, and mothers can devote time and attention to the bringing up of their children.

Besides these, there are other causes which tend to keep up their average duration of life, viz.:—

1st. There are few early marriages, and the number of births does not more than replace the deaths; thus, as there are fewer children, the number of deaths of children must be relatively smaller than in the country at large.

2nd. The Society of Friends contains an unusually large proportion of females, and as women live longer than men, the average age at death is thus slightly increased.

3rd. The relative number of Friends following unhealthy or laborious occupations is considerably less than in the country at large.

But, on the other hand, Friends are not distributed over the country in the proportion of the population, an undue proportion of them live in large and unhealthy towns, such as Manchester, Liverpool, Leeds, and Bradford, whilst there is a deficiency in the rural districts. On this account, therefore, the average duration of life is less than it would be if Friends were distributed as favourably as the population of the country at large.

The records of births, marriages, and deaths in the Society of Friends have been kept with great accuracy for a long period; and a small publication, "The Annual Monitor," is regularly issued, containing the names, ages, and residences of all the members who die in Great Britain and Ireland each year. The names of children, however, who die under one year are not inserted separately, but a summary of them is appended. This little book is not official; but the information contained is supplied to the editors by all the different registrars. The tables appended to this paper, embracing the years 1842-55 inclusive, are compiled from "The Annual Monitor."

The average age of death is not generally much to be relied on as an indication of the rate of mortality, being so much influenced by the relative number of births and deaths; yet, where these are nearly equal, and there is little fluctuation of numbers from emigration or other causes, the average age at death is trustworthy. The only really correct method of obtaining the rate of mortality is by comparing the annual number of deaths at each age with the number living at the same ages. An enumeration of the Society of Friends in Great Britain and Ireland was made 6th Month 30th, 1847, when there were found to be 18,733 persons living. The average number of deaths per annum for the last fourteen years is 357, therefore 19 per 1,000; or 1 in 51, died annually.

The average deaths per 1,000 living, for the whole country, is 22, or 1 in 45; but were the number of births and deaths equal it would be 1 in 41, or rather more than 24 deaths to 1,000 persons living.

The high rate of mortality in Manchester, Liverpool, and most of the large and densely populated towns, might be regarded as owing chiefly to the overwhelming preponderance of the lower classes. Many of these live in small and ill-ventilated houses, some in closed courts, and others live in cellars; and the greater number are employed at unwholesome or laborious occupations. Their lives are further shortened by addiction to intoxicating drink, by consuming badly-cooked food, by want of cleanliness, and by various privations; the employment of mothers in other than domestic duties, to the neglect of their offspring, tells seriously upon the mortality of children. Taking these things into consideration, it might be supposed that by attention to sanitary laws, the middle classes at any rate might attain as great an age in the town as in the country. To throw some light on this question, nineteen towns, containing a comparatively large number of Friends, have been selected, the age at death of each who died at one year

and upwards during the fourteen years 1842-55 recorded, and the average age at death of these is contrasted with the deaths of Friends in the remainder of Great Britain and Ireland during the same period. The number of children dying under one year has been divided in the proportion of the recorded deaths between one and five years; still the chance of inaccuracy on this ground is small, and cannot seriously affect the results.

Table 1 shows the deaths of males at various ages in the 19 selected towns—London, Bristol, Birmingham, Manchester, Liverpool, Leeds, Preston, Bradford, Newcastle, Sunderland, North and South Shields, Darlington, Hull, Belfast, Clonmel, Cork, Dublin, Limerick, and Waterford — total deaths, 726. Not a single death is recorded at 95 or upwards.

Table 2 shows the deaths of females in the same towns—total 933. Three women died at 95 and upwards.

Table 3 shows the deaths of men in Great Britain, and Table 5 in Ireland, exclusive of the nineteen towns before enumerated; showing, respectively, 1,240 and 62 deaths. Three men died at 95 and upwards.

Tables 4 and 6 show the deaths, as above, of females; being 744 in England, 191 in Ireland. Nine women died at 95 or above. It would thus appear that town life is unfitted for very old persons, and that there are more aged women than men; but as those who attain so great an age as ninety-five are very few, and the Society of Friends is small, not much reliance can be placed upon these deductions.

Tables 8 and 9 are summaries of the deaths of Friends, males and females, in the whole country, and present the following peculiarities. For every 100 boys and 100 girls born there died 14 boys and 8 girls under five years, the deaths of boys thus greatly preponderating. From the ages of 5 to 14 the deaths are nearly equal; from 15 to 24 there is

a greater fatality among men; from 25 to 54 among women; from 55 to 64 among men; from 65 to 74 it is about equal, and from 74 and upwards there being more aged women than men their deaths preponderate.

In tables 10 and 11 we see the contrast between the town and country, which is unfavourable to the former in every particular; and, as tender and young plants are soonest injured by ungenial influences, so we must expect to see the unhealthiness of towns prove especially fatal to children. We accordingly find that up to five years the deaths in towns are double those in the country. It must, however, here be borne in mind that the deaths under one year, though correct for the whole, are divided between the town and country in the proportion of the mortality of the subsequent four years. But, allowing a large margin for the chance of inaccuracy, the fatality of town life to children is excessive. Until the age of 54, when half of the town-born have died (while about two-thirds of the country-born are living), the mortality of the towns is in excess. After that age, as there are a greater number of survivors among the country population the deaths among them are more numerous, so that they appear at first sight to have a lower degree of health, but in the town these same individuals, whose deaths are enumerated, would have already died before that age. An examination of this table is conclusive that something more is wanted for successfully rearing children than suitable food, clothing, and shelter; plenty of pure air cannot with impunity be replaced by a polluted atmosphere, and no amount of nursing and medicine can compensate for the change.

Table 7 is an analysis of the deaths of children under one year, and the preponderance of the deaths of boys over girls is shown throughout. The deaths of boys under one month were 31 in excess of the girls; from one to three months, 8 in excess; from three to six, 14 in excess; six to twelve, 7 in

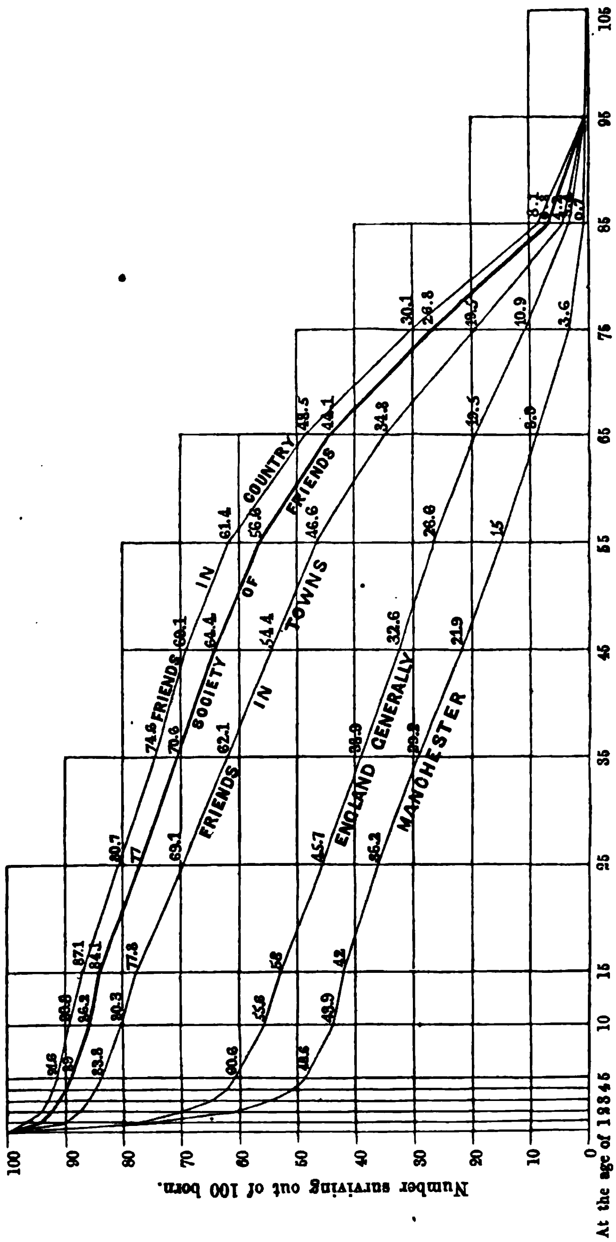
excess. This is the more worthy of notice as the number of females annually dying in the Society is greatly in excess of the males.

Table 13 shows the number of deaths and aggregate of years attained by those dying in the nineteen specified towns and elsewhere, also the average age at death; excluding children under one year. London, Birmingham, Cork, and Darlington appear to be healthy towns; the number dying in some of the other towns is too small to enable us to judge of their respective healthiness except in the aggregate.

Table 14 shows the average length of life in males and females, town and country. It appears that members of the Society of Friends, including both sexes, live on the average to the age of 51 years 1 month, which exceeds by 10 years the average of the people of England generally. The women however live longer than the men—the age of men being 48 years 1 month, and that of women 53 years 4 months. The difference between town and country is remarkably great, and of course in favour of the country. The average length of life in towns is 44 years 8 months, in the country 54 years 3½ months; so that the life of a person in the country is ten years longer or one-fourth more than that of one in a town, that is to say, if 100 persons were born in the town and 100 in the country, for each hour the former live the latter will live an hour and a quarter. When it is considered that the 1,659 persons whose deaths have been recorded in the towns, were as cleanly and temperate, as well fed and as warmly clad as their more fortunately situated friends who lived on the average ten years longer than they, attention is immediately turned to the polluted air of towns, to the effluvia and noxious gases arising from decaying animal and vegetable refuse, sewers, graveyards, and cesspools, to the sulphurous acid, carbonic acid, carbonic oxide, and carbon emanating from the chimneys of our manufactories, and to imperfect ventilation in and around our houses.

It is a serious thing to know that we, in towns, inhale with each breath a poison which is sapping our health and strength, and that though we may exercise the greatest care, we and our families are inevitably defrauded of one-fifth of our lives on the most favourable computation. When we further consider that in the nineteen enumerated towns the average duration of life is raised by several peculiarly healthy towns being included, as well as outskirts of towns, which are generally more healthy; and when we further remember that the list containing the remainder of deaths does not consist entirely of country districts, but includes Friends living in every unhealthy spot in the country, not computed in the nineteen enumerated towns, we cannot but conclude that the difference between town and country life is much greater than here shown. If we contrast the worst towns—Liverpool, with its unenviable notoriety of being the most unhealthy place in the kingdom, with 36 in the 1000 dying annually, Manchester with 33 in the 1000, Leeds 30, Bristol 29, Wigan 28, Bolton and Wolverhampton 27, &c., on the one hand, and the rural districts on the other, we cannot doubt that the difference would be at least 15 to 20 years, however attentive the inhabitants of the former might be with regard to their health.

The mortality of the Society of Friends, as before stated, contrasts very favourably with that of England generally. In the diagram the two are contrasted, the lines relating to the former being constructed on tables 10, 11, and 12, and the latter from the deaths of the people of England registered in the seven years 1838 to 1844, extracted from the Registrar-General's 8th Report. The most marked peculiarity consists in the difference between the deaths of young children, the number dying under one year, being of Friends 6.2, in England generally 22, or 3½ times more than in the Society of Friends. Under five years the deaths of Friends are only one-third of the average of England. The number of deaths


FRIENDS IN COUNTRY
SOCIETY OF FRIENDS
FRIENDS IN TOWNS
ENGLAND GENERALLY
MANCHESTER
Number surviving out of 100 born.
At the age of 1 2 3 4 5 10 15 25 35 45 55 65 75 85 95 105

Y

[*To prevent confusion in the diagram, the figures relating to deaths under one year are inserted separately in the following table.*]

INFANT MORTALITY.

		Friends Country.	Friends Average.	Friends Towns.	England Average.	Manchester Average.
Out of 100 Children born, the Number Surviving at....	1 year........	95·6	93.8	90.3	78.0	74.8
	2 „	94.0	91.8	87.6	70.0	61.0
	3 „	92.8	90.5	86.1	65.6	55.1
	4 „	92.1	89.7	84.9	62.7	51.3
	5 „	91.6	89.0	83.8	60.6	48.6

of Friends at 65 and upwards is 44 out of each 100 born; in England it is only 19½. In England generally, out of a given number born, one-half die before they attain 20 years; in Manchester one-half die before they attain 5 years. In the Society of Friends, 60 years must elapse before one-half die. At extreme old age, 95 and upwards, there would appear to be but little difference between the proportion dying among Friends and others; but as previously stated, the number attaining so great an age is very small, and not sufficient to warrant us in coming to any conclusion. As the Society of Friends does not increase in population the number of births is not in excess of the deaths, and the proportion of children is less than in England generally. This gives a somewhat fictitious increase to the average length of life, and also a corresponding diminution of infant deaths. Nevertheless this effect is small, and need scarcely enter into the calculation.

Another question naturally arises: Are unhealthy towns deteriorating or improving; have we in Manchester a greater or less expectancy of life than our fathers and grandfathers; is Manchester more or less healthy than it was half a century or a century ago, at least for such portion of the community as live in the most favourable manner, whether the *locality* is healthy or not? A very laborious transcription and classification of the records of the deaths of all persons interred

in the burial grounds of Friends within the limits of the Lancashire Quarterly Meeting, from the commencement of 1777 to the end of 1837, affords some interesting information on this subject. There is no reason to believe that the Society of Friends eighty years ago had fewer personal comforts, were more harrassed by the cares of business, were less cleanly, orderly, or temperate than at the present day, so we must not look to changes in these respects to account for any variation in the duration of life; they probably stood in the same relation to the community as they do now. It should be remarked that the list includes a few persons who died previous to 1777.

Table 15 gives a summary of this investigation. It will be observed throughout that the women live longer than the men, and on the average nearly two years. The proportion of women to men has steadily increased. To each 100 men who died in the twenty years ending 1797, there died 102 women; in the next twenty years 110; in the third twenty years 120. This great increase in the number of women arises from various causes, one of which may be, that more men leave the Society by disownment than women. Still this will only account in part for the remarkable increase of women, and the subject is worthy of investigation.

It must be observed that Table 15, containing the deaths of all persons interred in the burial grounds, includes a number of persons not in membership, but who were connected with Friends and attended their religious meetings; this reduces the average age of death. In Manchester the age at death of Friends only, for the twenty years ending 1856, was 41; for *all* interred in their burial grounds for the same time, 38. Thus we may add three years to each of the ages to form an idea of the age of Friends only.

LANCASTER appears to have been a very healthy town, the average duration of life eighty years ago was greater than that of Manchester at the present time. The duration of

life there has rapidly increased each twenty years. First twenty years, 39; second, 45; third, 49. The women who died in Lancaster during the last twenty years attained the age of 53½, and adding 3 for the correction it would be 56½; but one circumstance, affecting the duration of life of Friends in Lancaster, must be borne in mind, namely, that the number of Friends there has steadily decreased, this will tend to make the average age appear greater.

PRESTON. The number living in this town was so small that no safe comparison can be made of the relative duration of life at different periods; but if we take the total deaths in sixty years, we find the average duration of life 30 years, which must be considered low. More interments of those not members than the average took place in the graveyard of Friends at Preston; this will cause the average duration of life there to appear less.

In LIVERPOOL the records present a very singular appearance; they show for the first twenty years, duration of life 25 years; second, 41; third, 34½. This may, in part, be accounted for on the supposition that the number of Friends decreased after the first twenty years, and after the second twenty increased again. The number of deaths recorded would bear out this supposition, and thus the latter number would a little exceed the correct amount, and the former be a little less. Still this does not fully account for the difference.

It would be hazardous and unsafe on this authority *alone* to assume that the sanitary condition of Liverpool improved up to a certain date; after which, that the extension of docks, the free immigration of the Irish with their habits, the rapid extension of the town without an efficient system of sewers, together with other causes, caused it to deteriorate.

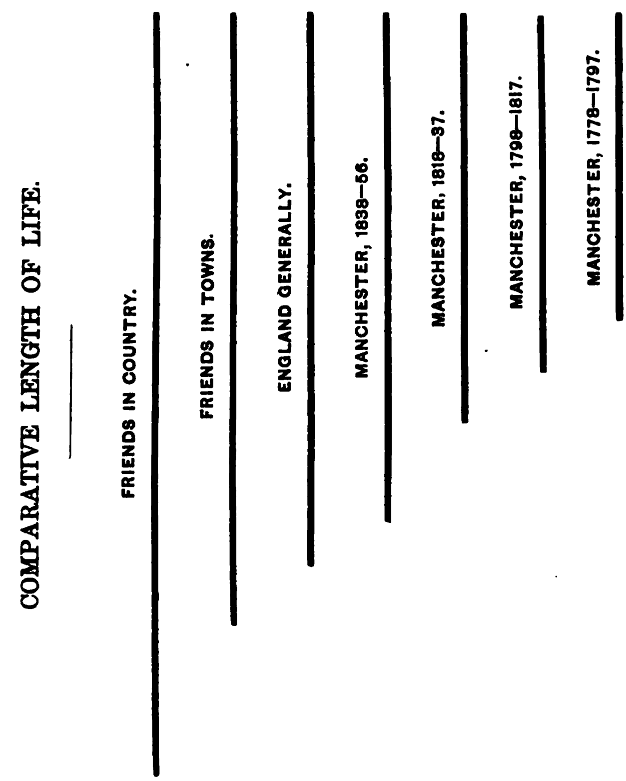
Bad as the statistics of mortality make Manchester appear when compared with other towns, it is cheering to see a progressive and steady increase in the duration of life, not retarded by the increase of the population, nor checked by the yearly increasing smoke and sulphur which we are told are so deadly in their effects. No doubt these are bad enough, but it is pleasant to turn to the brighter side and

to know that we are mending. The result for those interred in the burial grounds of Friends is as follows:—

20 years	ending	1797,	average	duration of life,	25	years.
,,	,,	1817,	,,	,,	28½	,,
,,	,,	1837,	,,	,,	31½	,,
,,	,,	1856,	,,	,,	38	,,

How marked is the step in the last twenty years, being as much as in the previous forty years! New wide streets, plenty of good water, the removal of old cottage property in the heart of the town, clean streets, good sewers, and extra-mural interments have had their share, no doubt, in raising the average; and are, it is to be expected, destined to improve it still more.

The following lines show at a glance by their length the average duration of life of some of the classes under consideration:—



The deaths in OTHER PARTS of Lancashire, not included in the four towns enumerated, present, at the first sight, a strange appearance. The average age steadily falls—from 43 to 37 and from 37 to 34½—yet this is perfectly consistent, and is one other striking proof of the unhealthy influences in our towns. Eighty to one hundred years ago many of our unhealthy smoky towns were then comparatively healthy hamlets, and at that time nearly all the people who were not included in the largest towns, were living in the country; but now, Wigan, Bolton, Prescot, Warrington, Oldham, and others, have become densely peopled. Thus we cannot assume that the rural district is more unhealthy, but being partly converted into a town district the variation is accounted for.

In the WHOLE OF LANCASHIRE there seems but little change in the duration of life in the last sixty years. The deleterious results arising from the increasing density of the population, and the extension of the cotton manufacture, accompanied with the combustion of so much coal, seem to have been nearly counteracted by the increased knowledge of the laws of health.

A careful examination of the tables will suggest many other points of interest besides those here briefly indicated, but even a cursory glance will show that life may be much prolonged by temperance and care. It will be equally evident that towns are unfavorable to long life, and especially unfitted for young children, whose life is always precarious and who are so easily affected by injurious influences. Still we find that it is quite possible for a town to increase in healthiness whilst it increases in size, so we may hope that by a further knowledge of the laws of health, and more especially by further attention to their requirements, the time will come when the "Health of Towns" may be a reality, not a name.

TABLE No. 1.

SOCIETY OF FRIENDS.

Nineteen Towns in Great Britain and Ireland.—Deaths at different Ages for the Fourteen Years, 1842–55, *inclusive.*

MALES.

	Total Deaths.	AGES AT DEATH.																
		Und'r 1 Yr.	1	2	3	4	Und'r 5 Yrs.	5-	10-	15-	25-	35-	45-	55-	65-	75-	85-	95 and upwards.
London	..	..	7	4	..	1	12	6	6	10	4	10	12	23	31	19	7	..
Bristol	..	..	1	..	..	..	1	..	..	5	6	3	3	9	13	8	1	..
Birmingham	..	..	2	..	..	..	2	..	..	3	2	3	1	..	4	5	..	..
Manchester	..	..	..	..	3	..	3	3	1	5	4	5	4	12	6	5	2	..
Liverpool	..	..	1	..	..	1	2	1	2	4	3	6	5	4	8	5	1	..
Leeds	..	..	..	..	1	..	1	3	1	5	4	1	3	7	5	9	2	..
Preston	..	..	..	4	2	..	6	..	2	..	1	1	..	..	1	1	1	..
Bradford	..	..	..	..	..	..	..	1	1	6	1	2	3	3	3	4	..	..
Newcastle	..	..	..	..	..	2	2	1	1	2	3	3	..	1	2	2	..	..
Sunderland	..	..	1	..	..	..	1	2	..	4	..	..	..	4	2	2	..	..
N. and S. Shields	..	..	1	1	..	..	2	1	1	2	3	..	1	1	2	..	..	..
Darlington	..	..	..	1	..	..	1	2	1	2	1	3	1	6	3	5	2	..
Hull	..	..	..	..	..	..	..	..	1	..	1	..	1	1	2	4	..	..
Belfast	..	..	..	..	..	..	..	1	..	2	3	2	3	1	..	3	1	..
Clonmel	..	..	..	..	2	1	3	3	1	2	1	2	2	2	5	3	..	..
Cork	..	..	1	1	1	..	3	3	2	..	1	3	4	5	7	6	..	..
Dublin	..	..	2	2	1	3	8	2	1	15	8	5	7	7	2	5	2	..
Limerick	..	..	..	2	..	..	2	..	..	3	..	..	5	2	1	..	..	..
Waterford	..	..	1	..	..	..	1	..	2	6	..	2	..	..	3	4	1	..
	726	98	17	15	10	8	148	29	23	76	46	51	55	88	100	90	20	..

TABLE No. 2.

SOCIETY OF FRIENDS.

Nineteen Towns in Great Britain and Ireland.—Deaths at different Ages for the Fourteen Years, 1842-55, *inclusive.*

FEMALES.

	Total Deaths.	AGES AT DEATH.																
		Und'r 1 Yr.	1	2	3	4	Und'r 5 Yrs.	5-	10-	15-	25-	35-	45-	55-	65-	75-	85-	95 and upwards.
London	...	...	6	1	2	2	11	4	5	14	13	16	20	26	41	41	14	2
Bristol	...	...	...	1	...	...	1	2	2	5	3	8	7	6	16	23	7	...
Birmingham	...	...	2	...	...	1	3	3	...	3	4	1	7	3	9	9	2	..
Manchester	...	...	3	1	1	2	7	1	1	4	4	3	5	9	7	8	3	...
Liverpool	...	...	5	2	1	2	10	...	2	3	7	2	3	8	17	12	...	...
Leeds	...	...	1	...	...	...	1	2	2	3	3	5	6	3	2	2	1	...
Preston	...	...	1	...	2	1	4	...	...	3	...	...	1	1	3	7	...	...
Bradford	...	...	3	...	...	...	3	1	4	2	2	7	...	4	2	1	...	..
Newcastle	...	..	...	1	1	2	4	2	...	1	2	4	2	5	5	4	3	...
Sunderland	...	...	...	1	...	..	1	2	...	1	4	3	...	1	6	3	1	...
N. and S. Shields	...	...	...	...	1	...	1	1	1	2	1	1	3	2	1	...	1	...
Darlington	...	...	...	...	...	...	...	1	...	2	4	2	...	3	2	8	2	...
Hull	...	...	1	...	...	...	1	...	2	3	3	...	2	3	7	6	...	...
Belfast	...	...	...	1	...	...	1	...	...	5	4	2	1	...	4	3	3	...
Clonmel	...	...	2	...	...	...	2	...	...	3	4	3	3	5	5	4	...	1
Cork	...	...	2	...	...	...	2	3	...	2	4	9	5	8	9	8	...	...
Dublin	...	...	3	1	1	...	5	7	...	9	7	6	6	11	11	16	3	...
Limerick	...	...	...	...	1	...	1	...	...	2	...	...	3	2	...	...	2	...
Waterford	...	...	...	1	...	...	1	1	1	1	2	5	2	7	3	7	4	...
	933	64	29	10	10	10	123	30	20	68	71	77	76	107	150	162	46	3

TABLE No. 3.

SOCIETY OF FRIENDS.

Great Britain, exclusive of Thirteen Towns.—Deaths at different Ages for the Fourteen Years, 1842-55, *inclusive.*

MALES.

	AGES AT DEATH.																		
	Total Deaths.	Und'r 1 Yr.	1	2	3	4	Und'r 5 Yrs.	5-	10-	15-	25-	35-	45-	55-	65-	75-	85-	95 and upwds	
1842	...	...	5	3	3	...	11	3	1	9	2	3	9	16	18	20	9	...	1842
1843	...	...	2	4	...	...	6	3	1	12	8	3	8	10	16	17	5	...	1843
1844	...	...	2	3	...	2	7	...	1	4	4	11	8	10	15	14	2	...	1844
1845	...	...	4	...	2	...	6	2	1	5	4	4	4	15	24	15	1	1	1845
1846	...	...	2	3	...	...	5	4	...	6	7	3	4	15	20	21	3	...	1846
1847	...	...	2	1	2	...	5	2	...	4	4	3	5	8	28	23	4	...	1847
1848	...	...	2	1	1	...	4	4	3	9	4	2	5	14	11	17	8	...	1848
1849	...	...	2	...	1	...	3	1	1	4	3	4	8	10	17	14	7	...	1849
1850	...	...	1	2	...	2	5	...	...	5	3	4	6	15	15	16	9	...	1850
1851	...	...	1	...	1	...	2	2	3	8	4	3	9	10	14	26	5	...	1851
1852	...	...	2	...	1	1	4	2	2	8	6	3	4	16	16	16	4	...	1852
1853	...	...	3	...	...	1	4	1	2	4	2	4	10	16	19	14	4	1	1853
1854	...	...	1	...	2	1	4	...	2	5	6	4	9	10	20	19	4	...	1854
1855	...	...	2	...	...	...	2	1	4	3	2	6	5	13	14	25	8	...	1855
	1240	73	31	17	13	7	141	25	21	86	59	57	94	178	247	257	73	2	

TABLE No. 4.

SOCIETY OF FRIENDS.

Great Britain, exclusive of Thirteen Towns.—Deaths at different Ages for the Fourteen Years, 1842–1855, *inclusive.*

FEMALES.

	Total Deaths.	AGES AT DEATH. Und'r 1 Yr.	1	2	3	4	Und'r 5 Yrs.	5-	10-	15-	25-	35-	45-	55-	65-	75-	85-	95 and upwds	
1842	...	...	...	1	1	2	4	3	1	2	12	7	12	14	26	26	10	1	1842
1843	...	...	5	2	...	2	9	5	1	9	10	12	9	16	18	31	12	...	1843
1844	...	...	...	1	...	...	1	3	2	10	9	5	10	20	23	26	12	...	1844
1845	...	...	1	1	...	...	2	3	6	6	7	11	14	14	24	27	19	2	1845
1846	...	...	1	1	...	2	4	1	3	7	4	9	5	12	23	26	13	...	1846
1847	...	...	...	1	1	1	3	4	7	4	11	4	6	21	28	36	8	1	1847
1848	...	...	1	4	1	...	6	2	3	10	9	6	11	13	18	35	9	...	1848
1849	...	...	...	1	2	...	3	2	6	8	7	11	9	15	26	26	17	1	1849
1850	...	...	2	...	...	...	2	3	...	1	7	3	7	16	21	25	8	...	1850
1851	...	...	2	...	...	1	3	4	2	4	10	11	8	14	10	28	6	...	1851
1852	...	...	1	1	...	1	3	1	2	14	12	5	10	12	22	28	8	...	1852
1853	...	...	...	...	1	...	1	1	1	6	8	6	9	11	25	34	9	...	1853
1854	...	...	2	1	...	...	3	3	5	5	15	9	11	18	22	34	10	3	1854
1855	...	...	1	3	2	...	6	9	2	5	2	8	14	16	26	32	11	...	1855
	1744	55	16	17	8	9	105	44	41	91	123	107	135	212	312	414	152	8	

TABLE No. 5.

SOCIETY OF FRIENDS.

Ireland: exclusive of Six Towns.—Deaths at different Ages for the Fourteen Years, 1842-55, *inclusive.*

MALES.

	Total Deaths.	AGES AT DEATH.																	
		Und'r 1 Yr.	1	2	3	4	Und'r 5 Yrs.	5-	10-	15-	25-	35-	45-	55-	65-	75-	85-	95 & upwds	
1842	..	...	...	1	...	1	2	...	...	1	..	...	...	...	2	1	...	...	1842
1843	...	...	1	2	...	...	3	...	...	4	1	...	1	...	...	3	...	...	1843
1844	...	...	...	...	...	...	...	1	...	3	1	2	...	2	1	2	1	...	1844
1845	...	...	...	...	..	...	...	1	1	2	...	...	1	1	2	2	...	1	1845
1846	...	...	...	...	...	...	...	...	1	...	2	...	1	1	2	...	...	...	1846
1847	...	...	1	...	...	...	1	1	...	1	2	1	...	2	2	5	1	...	1847
1848	...	...	...	...	...	...	...	...	1	2	...	1	1	...	3	3	1	...	1848
1849	..	...	...	...	...	...	...	...	...	...	...	1	2	2	4	2	2	...	1849
1850	...	...	1	...	1	...	2	...	...	1	...	...	2	1	...	3	...	...	1850
1851	...	...	1	...	...	...	1	1	...	1	1	1	...	1	1	...	1	...	1851
1852	...	..	...	...	...	...	...	1	...	...	...	...	...	1	1	1	...	...	1852
1853	...	..	...	...	...	...	...	...	1	...	...	2	...	2	2	3	1	...	1853
1854	...	...	...	...	...	...	...	1	1	...	1	...	1	2	2	4	1	...	1854
1855	...	...	...	1	...	...	1	1	...	2	...	1	...	3	2	2	1	...	1855
	162	14	4	4	1	1	24	7	5	17	8	9	9	18	24	31	9	1	

TABLE No. 6.

SOCIETY OF FRIENDS.

Ireland, exclusive of Six Towns.—Deaths at different Ages for the Fourteen Years, 1842–1855, *inclusive.*

FEMALES.

	Total Deaths.	AGES AT DEATH. Und'r 1 Yr.	1	2	3	4	Und'r 5 Yrs.	5-	10-	15-	25-	35-	45-	55-	65-	75-	85-	95 & upwds	
1842	...	...	...	...	...	...	...	1	...	2	1	1	1	...	1	1	4	...	1842
1843	...	...	1	...	...	...	1	...	1	2	1	1	2	3	4	4	1	...	1843
1844	...	...	...	1	...	...	1	...	...	...	3	...	1	2	3	4	...	...	1844
1845	...	...	...	...	...	...	...	...	...	...	1	...	3	3	1	1	4	...	1845
1846	...	...	...	1	...	...	1	...	...	2	...	3	...	5	2	3	3	..	1846
1847	...	...	...	...	...	...	...	...	2	3	1	2	2	...	2	4	3	...	1847
1848	...	...	...	...	1	...	1	...	...	3	1	2	...	...	4	2	2	...	1848
1849	...	...	...	...	...	...	...	...	...	1	...	...	2	1	2	3	2	...	1849
1850	...	...	...	...	1	1	2	1	...	1	2	...	3	1	3	1	2	1	1850
1851	...	...	...	...	...	...	...	...	1	...	1	1	1	1	...	1	...	...	1851
1852	...	...	..	...	...	...	...	...	...	...	...	1	1	2	2	3	...	...	1852
1853	..	...	...	...	...	...	...	...	...	1	1	...	...	...	3	1	1	...	1853
1854	...	...	...	...	...	...	...	...	1	...	2	...	1	2	2	2	1	...	1854
1855	...	...	...	...	...	..	...	...	1	...	1	...	1	2	2	5	...	...	1855
	191	6	1	2	2	1	12	2	6	15	15	11	18	22	31	35	23	1	

TABLE No. 7.

SOCIETY OF FRIENDS.

Great Britain and Ireland.—Deaths of Children under 1 *Year, for* 14 *Years,* 1842–1855, *inclusive.*

	Males.					Females.				
	Total Deaths.	Und'r 1 Mo.	From 1 to 3 Mos.	From 3 to 6 Mos.	From 6 to 12 Mos.	Total Deaths.	Und'r 1 Mo.	From 1 to 3 Mos.	From 3 to 6 Mos.	From 6 to 12 Mos.
1842	8	2	..	2	4	8	3	1	1	3
1843	19	7	5	3	4	9	1	4	2	2
1844	12	4	5	..	3	12	2	1	4	5
1845	22	7	2	6	7	4	..	1	1	2
1846	20	6	6	3	5	9	..	3	3	3
1847	11	..	3	3	5	15	1	2	3	9
1848	13	3	1	7	2	10	4	..	..	6
1849	14	2	1	5	6	10	2	2	1	5
1850	5	1	2	1	1	8	1	3	3	1
1851	17	4	4	3	6	11	..	4	4	3
1852	10	5	..	1	4	5	1	..	1	3
1853	13	7	2	3	1	8	4	..	2	2
1854	11	4	2	3	2	9	3	4	..	2
1855	10	2	1	2	5	7	1	1	3	2
	185	54	34	42	55	125	23	26	28	48

TABLE No. 8.

SOCIETY OF FRIENDS.

SUMMARY.—*Deaths at different Ages in Great Britain and Ireland for the Fourteen Years*, 1842-55.

MALES.

	Total Deaths.	Und'r 1 Yr.	1	2	3	4	Und'r 5 Yrs.	5-	10-	15-	25-	35-	45-	55-	65-	75-	85-	95 & upwd.
		AGES AT DEATH.																
Nineteen Towns ...	726	98	17	15	10	8	148	29	23	76	46	51	55	88	100	90	20	...
Gt.Britain,excluding thirteen towns ...	1240	73	31	17	13	7	141	25	21	86	59	57	94	178	247	257	73	2
Ireland, excluding six towns	162	14	4	4	1	1	24	7	5	17	8	9	9	18	24	31	9	1
Total	2128	185	52	36	24	16	313	61	49	179	113	117	158	284	371	378	102	3
Per centage	100	8.7	2.5	1.7	1.1	.7	14.7	2.9	2.3	8.5	5.3	5.5	7.5	13.4	17	18	4.8	.1

TABLE No. 9.

FEMALES.

	Total Deaths.	Und'r 1 Yr.	1	2	3	4	Und'r 5 Yrs.	5-	10-	15-	25-	35-	45-	55-	65-	75-	85-	95 & upwd.
		AGES AT DEATH.																
Nineteen Towns ...	933	64	29	10	10	10	123	30	20	68	71	77	76	107	150	162	46	3
Gt.Britain,excluding thirteen towns ...	1744	55	16	17	8	9	105	44	41	91	123	107	135	212	312	414	152	8
Ireland, excluding six towns	191	6	1	2	2	1	12	2	6	15	15	11	18	22	31	35	23	1
Total	2868	125	46	29	20	20	240	76	67	174	209	195	229	341	493	611	221	12
Per centage	100	4.4	1.6	1.0	.7	.7	8.4	2.7	2.4	6.0	7.4	6.8	8.0	11.8	17.0	21.4	7.7	.4

TABLE No. 10.

SOCIETY OF FRIENDS.

SUMMARY.—*Deaths at different Ages in* NINETEEN TOWNS *in Great Britain and Ireland for Fourteen Years,* 1842–1855, *inclusive.*

	Total Deaths.	AGES AT DEATH. Und'r 1 Yr.	1	2	3	4	Und'r 5 Yrs.	5-	10-	15-	25-	35-	45-	55-	65-	75-	85-	95 & upwd.
Males	726	98	17	15	10	8	148	29	23	76	46	51	55	88	100	90	20	...
Females	933	64	29	10	10	10	123	30	20	68	71	77	76	107	150	162	46	3
Total	1659	162	46	25	20	18	271	59	43	144	117	128	131	195	250	252	66	3
Per centage	100	9.7	2.7	1.5	1.2	1.1	16.2	3.5	2.5	8.7	7.0	7.7	7.8	11.8	15.3	15.3	4.0	0.2

TABLE No. 11.

SUMMARY.—*Deaths at different Ages in* GREAT BRITAIN AND IRELAND, *excluding Nineteen Towns, for the Fourteen Years* 1842–55, *inclusive.*

	Total Deaths.	AGES AT DEATH. Und'r 1 Yr.	1	2	3	4	Und'r 5 Yrs.	5-	10-	15-	25-	35-	45-	55-	65-	75-	85-	95 & upwd.
Males	1402	87	35	21	14	8	165	32	26	103	67	66	103	196	271	288	82	3
Females	1935	61	17	19	10	10	117	46	47	106	138	118	153	234	343	449	175	9
Total	3337	148	52	40	24	18	282	78	73	209	205	184	256	430	614	737	257	12
Per centage	100	4.4	1.6	1.2	0.7	0.5	8.4	2.3	2.2	6.4	6.1	5.5	7.7	12.9	18.4	22.0	7.7	0.4

Per centage	100	6.2	2	1.3	.8	.7	11	2.8	2.1	7.1	6.4	6.2	7.8	12.5	17.3	20	6.5	.3

TABLE No. 13.

SOCIETY OF FRIENDS.

Average Age at Death (EXCLUDING CHILDREN WHO HAVE DIED UNDER ONE YEAR), *for the* 14 *Years*, 1842–55 *inclusive.*

PLACES.	Males.			Females.		
	No. of Deaths.	Aggregate Yrs.	Average Age.	No. of Deaths.	Aggregate Yrs.	Average Age.
			Yrs. Mos.			Yrs. Mos.
Gt. Britain & Ireland	1943	102360	52 8	2743	152888	55 9
Gt. Britain, excluding large towns.........	1167	65614	56 3	1690	97270	57 7
Ireland, excluding large towns.........	148	7668	51 10	185	10599	57 4
Total large towns ...	628	29078	46 4	868	45019	51 10
London*	140	7086		207	11539	
Bristol	50	2676		80	4786	
Birmingham	20	964		44	2338	
Manchester	50	2339		52	2508	
Liverpool	41	1971		64	3092	
Leeds...............	41	2101		30	1260	
Preston	13	344		19	940	
Bradford............	24	1100		26	1090	
Newcastle	16	583		32	1569	
Sunderland.........	15	636		22	1063	
N. & S. Shields ...	13	396		14	538	
Darlington	27	1412		24	1403	
Hull	10	614		27	1421	
Belfast	16	628		23	1119	
Clonmel	24	1015		30	1518	
Cork	34	1649		50	2518	
Dublin	62	2260		81	3918	
Limerick	13	490		10	492	
Waterford	19	814		33	1907	

* To prevent conclusions being hastily drawn the average of each town is not given, as great importance can be attached only to the aggregate result.

TABLE No. 14.

SOCIETY OF FRIENDS.

SUMMARY.—*Average length of Life, Great Britain and Ireland, for the Fourteen Years* 1842–1855, *inclusive.*

	No. of Deaths.	Years.	Months.
Both Sexes	4996	51	1
Total Males	2128	48	1
„ Females	2868	53	4
Males, 19 Towns	726	40	6
„ remainder Great Britain	1240	52	11
„ remainder Ireland	162	47	4
Females, 19 Towns	933	48	3
„ remainder Great Britain	1744	55	8
„ remainder Ireland	191	55	6
Total Towns	1659	44	8
„ Country	3337	54	3½

TABLE No. 15.

SOCIETY OF FRIENDS.

Interments in the Burial Grounds of Friends within the Quarterly Meeting of Lancashire.

	MALES.		FEMALES.		TOTAL.	
	No. of Deaths.	Av'rage Age.	No. of Deaths.	Av'rage Age.	No. of Deaths.	Av'rage Age.
Lancaster.						
1778 to 1797 inclusive	99	35½	103	42½	202	39
1798 to 1817 "	64	38	73	50½	137	45
1818 to 1837 "	30	41	52	53½	82	49
Preston.						
1778 to 1797 inclusive	31	19	16	24	47	21
1798 to 1817 "	30	24½	22	37½	52	30
1818 to 1837 "	36	39	41	33	77	35½
Liverpool.						
1778 to 1797 inclusive	106	24½	101	26	207	25
1798 to 1817 "	81	45	95	41½	176	41
1818 to 1837 "	124	35½	138	33	262	34½
Manchester.						
1778 to 1797 inclusive	114	25	111	25	225	25
1798 to 1817 "	129	27½	141	29½	270	28½
1818 to 1837 "	159	27½	182	35	341	31½
1838 to 1856 "	173	36	155	40½	328	38
Remainder of Lancashire Q. M.						
1778 to 1797 inclusive	351	39½	384	47½	735	43
1798 to 1817 "	306	38½	335	35½	641	37
1818 to 1837 "	384	33½	395	35	779	34½
Total of Lancashire Q.M.						
1778 to 1797 inclusive	701	33½	715	39	1416	36½
1798 to 1817 "	610	36	666	36½	1276	36½
1818 to 1837 "	733	33	808	36	1541	34½

XII.—*On the Formation of Indigo-blue.* Part II.

By Edward Schunck, Ph.D., F.R.S.

[*Read April 15th,* 1856.]

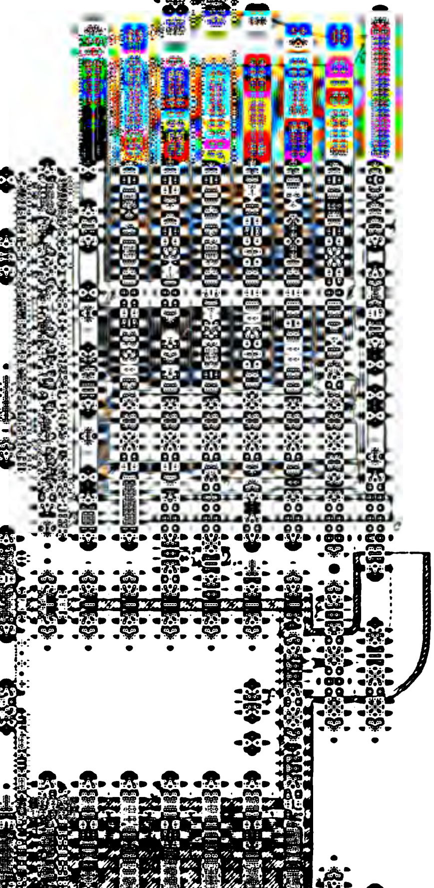
In the first part of this memoir I announced the discovery of a peculiar substance contained in the leaves of the *Isatis tinctoria,* to which, as I there showed, the indigo-blue obtained in the usual process of treating the plant owes its origin. Having applied to this substance the name of *Indican,* I proceeded to give a general account of its properties and of the process of decomposition which it undergoes when subjected to the action of strong acids. I now propose to present a more detailed account of the properties of this substance and especially of the products of decomposition derived from it.

In continuing my experiments I soon arrived at the conclusion, that the different methods of preparing indican, of which I had in the first part of this paper given a description, though they sufficed for the preparation of small quantities, were not well adapted for obtaining in a state of purity the larger quantities of the substance which I found to be necessary for the purposes of investigation. The great difficulty in the preparation of indican arises, as I have before stated, from the extreme facility with which it is decomposed, when its solutions, especially the watery one, are heated, a process of decomposition which is rapidly completed at a temperature a little below that of boiling water, and takes place even at the ordinary temperature of the atmosphere, when the evaporation occupies some time. This circumstance

renders its necessary to avoid distilling the solutions or evaporating them at any but the usual temperature. On the other hand, the length of time necessary for the spontaneous evaporation of the watery solution produces in a great measure the same effect as the evaporation of the solution at a higher temperature during a shorter period of time. It therefore became necessary to devise some means of producing a more rapid evaporation of these solutions without the application of artificial heat. This object was attained by means of a simple apparatus, in which a rapid current of air was made to pass over a large surface of the liquid to be evaporated, and which may be described in a few words.

The solution to be evaporated is poured into a dish or tray of block tin about 16 inches square with perpendicular sides 2 inches deep, and capable therefore of containing when full nearly two gallons of liquid. The dish is placed on a shelf fixed at a convenient height in a wooden box of which *a b c d*, Fig. 1, represents the front. This box is closed at the two sides, but open at the front and back from the shelf upwards. It must be sufficiently wide to allow the dish to slide easily in and out, but from front to back it must be so deep as to leave a space of about ½ an inch between the front and the dish. At the distance of about 1½ inch from the back of the box there is fixed in a perpendicular position a board *f*, the upper and side edges of which are firmly attached to the top and sides of the box. The lower edge of this board is about on a level with the upper edge of the tin dish and is accurately fitted to a shelf *g*, which is suspended by means of two upright pieces of wood *h h*, 2¾ inches deep, resting on two ledges *i i*, fixed to the sides of the box. The spaces between *h h* and the side walls of the box must be sufficiently wide to allow the sides of the tin dish to move easily up and down in them. By means of supports *n n* inserted between the tin dish *e* and the shelf *o* the former may be raised so as

... it close to the ... thin the dish. ... p left between ... sed as tightly ... s to cause the ... to sweep over ... t of the box is



... ith muslin and ... es, which in a

great measure prevents the dust which is carried along by the current of air from being conveyed into the liquid. The apparatus is now placed so as to make the back fit as closely as possible to the wall *q r*, Fig. 2, in which there is an opening *s* communicating with a steam-boiler flue, or the back of the box may be closed with a piece of wood, having an opening communicating by means of a pipe with the flue. The section Fig. 2 shows the direction taken by the air in passing over the surface of the liquid. As the liquid evaporates the dish is raised by means of additional supports, so as again to bring the surface of the former close to the shelf *g* and thus confine the current within a narrow space. The current of air which I employed, and which was sufficiently rapid to cause a constant ripple on the surface of the liquid, was produced by the draught of a steam-boiler flue, which carried away the products of combustion from several large fires. I think it probable, however, that the same effect might be produced by causing the whole of the air necessary for the supply of an ordinary stove or close fireplace to pass through the apparatus. By means of the current of air at my disposal I was enabled to evaporate in this apparatus about one pint of water in the course of twenty-four hours at a temperature not exceeding 10° C., the temperature of the water being kept by means of the rapid evaporation rather lower than that of the atmosphere. The evaporation of a gallon of spirits of wine by the same means occupied only a few hours.

In preparing indican the course of proceeding which I adopt is as follows.* The dried woad leaves are reduced to

* In the course of the investigation I had an opportunity of confirming a statement made by the authors, who have described the cultivation of the woad plant and the preparation of the dye made from it, viz., that the first crop of leaves obtained during the first year's growth of the plant is richest in colouring matter, and that each successive crop yields less than the preceding one. This may perhaps be ascribed to the lower temperature prevailing during the latter part of the year. Nevertheless if the roots be left in the ground through the

a powder, which is passed through a hair sieve, in order to separate the leaf-stalks and ribs of the leaves, and an extract of this powder is then made in a displacement apparatus with cold alcohol in the usual manner. The extract, which may be made stronger by passing it through fresh quantities of powder, has a lively dark green colour. It is evaporated in the apparatus just described, a little water being previously added to it, in order to facilitate the separation of the fatty matter. After a few hours there is found at the bottom of the evaporating dish a dark green layer, consisting of fat and green colouring matter, covered by a light brown watery liquid. The latter is poured off, filtered, agitated with a quantity of freshly precipitated oxide of copper, and filtered again. It now appears of a dark green colour from oxide of copper in solution. The latter having been removed by means of sulphuretted hydrogen, the filtered liquid, which is now quite clear and of a light yellow colour, is evaporated again in the same apparatus, when it leaves a brown syrup. This syrup contains besides indican some products of decomposition of the latter. On being treated with cold alcohol only a portion of it is dissolved, a part remaining undissolved in the form of a brown glutinous substance, which is a product of the combined action of water and oxygen on indican, and which will be

winter, though the plant in the ensuing year seems to have lost none of its vigour, as may be seen by the size and abundance of rich glaucous leaves which it puts forth, and the quantity of flower stems bearing numbers of flowers, and then of seeds which it sends up, still the leaves are as poor in colouring matter as those of the preceding autumn. The inferior quality of the dye produced from the second year's leaves, which in Thuringia went by the name of "Kompts-waid" (see Schreber's Beschreibung des Waids) was well known to the growers of woad in former times.

Woad is still employed by the woollen dyers in this country, but what useful purpose it answers in preference to an equivalent quantity of indigo, I am unable to say. A specimen of the drug, as used by a woollen dyer, which I examined, contained no trace of indigo-blue. If its use be merely to act as a ferment and reducing agent on the indigo employed at the same time, as is very probable, its place might be supplied by rotten cabbage leaves or decaying vegetable matter of any kind.

more fully described below. The alcoholic liquid after being poured off is mixed with about twice its volume of ether, when it becomes milky and deposits a substance of a syrupy consistence, which contains an additional quantity of the body just referred to and also some of the peculiar kind of sugar which is formed by the decomposition of indican. After the mixture has stood for several hours there is usually found deposited on the surface of the syrup and attached to the sides of the glass a quantity of white crystalline needles, which also consist of a product of the decomposition of indican. After the ethereal liquid has become clear, it is poured on a filter and then evaporated as before, when it leaves a clear brown syrup, consisting of indican in as high a state of purity as I have been able to obtain it. The only impurity which may still attach to the indican as thus prepared is a small quantity of fatty matter, the last traces of which it is extremely difficult to remove. When an alcoholic extract of woad is evaporated and water is added to the residue, the filtered liquid, though it may appear tolerably clear, still contains a quantity of fatty matter, in a state either of solution or, as seems more probable, of mechanical suspension. On adding acid to it, this fatty matter separates in greenish masses, which melt when the liquid is heated. The greatest part of this fatty matter is carried down by the oxide of copper used in the process just described, and the remainder is generally removed, when sulphuretted hydrogen is passed through the filtered liquid, by the precipitated sulphuret of copper. A little more separates occasionally in small white grains during the evaporation of the liquid filtered from the sulphuret of copper, especially if the temperature of the current of air passing over its surface be low, as in winter. If, however, the residue left after evaporation of the alcoholic extract be stirred up and agitated for some time with water, so large a quantity of fatty matter becomes suspended in the liquid as to render its separation without de-

composition of the indican almost impossible, and it is for this reason that I have found it advisable always to add water to the alcoholic extract of the leaves before evaporation, and to pour off the watery liquid from the undissolved chlorophyll and fatty matter, instead of evaporating the extract by itself and then stirring up the residue with water.

I have very little to add to the description formerly given of indican and its properties. It is always obtained in the form of a transparent light brown syrup, and it cannot be separated from the water which it still retains in this state without decomposition. Its watery solution has a yellow colour and a purely bitter taste. Even in the highest state of purity in which it can be obtained it produces when dissolved in water a slightly acid reaction on litmus paper. Whether this reaction is peculiar to it, or whether it is a consequence of a commencing decomposition of this easily decomposable substance, I am unable to decide. After being prepared in the manner just described it yields when subjected in small quantities to the action of acids, indigo-blue, indirubine and sugar, with mere traces of other products of decomposition. When the same process however is performed on a somewhat larger scale, other products make their appearance from causes which I shall presently explain.

The new experiments which I have made to determine the composition of indican confirm the conclusions at which I arrived in the first instance and which are contained in the first part of this paper. Being unable to obtain the substance itself in a state fit for analysis, I was obliged, as before, to have recourse to its compound with oxide of lead. This compound was prepared in the following manner. Pure indican was dissolved in cold alcohol, and the solution was mixed with a small quantity of an alcoholic solution of acetate of lead and filtered from the precipitate, which was generally of a somewhat dirty yellow colour. On now adding to the liquid an excess of acetate of lead a bright sulphur-yellow

precipitate fell, which was collected on a filter, washed with alcohol and then dried, at first in vacuo, and then for a few hours in the waterbath. This precipitate was employed in the analyses I. and II. The liquid filtered from this precipitate gave with a little ammonia a second precipitate of a rather paler yellow colour, which was collected on a filter, washed with alcohol and dried in the same manner as the first. The analyses III. and IV. were made with this precipitate. The following results were obtained.

I. 1.0120 grm. burnt with oxide of copper and chlorate of potash gave 0.7590 grm. carbonic acid and 0.1885 water.

1.0330 grm. burnt with soda-lime gave 0.0580 grm. platinum.

0.6385 grm. gave 0.5140 grm. sulphate of lead.

II. 0.8845 grm. gave 0.5040 grm. carbonic acid.

1.1100 grm. burnt with soda-lime gave 0.1260 grm. chloride of platinum and ammonium.

0.3490 grm. gave 0.3260 grm. sulphate of lead.

III. 1.3315 grm. gave 0.9380 grm. carbonic acid and 0.2535 water.

1.6295 grm. gave 0.2145 grm. chloride of platinum and ammonium.

0.8550 grm. gave 0.7005 grm. sulphate of lead.

IV. 0.9520 grm. gave 0.6345 grm. carbonic acid and 0.1715 water.

1.2490 grm. gave 0.0640 grm. platinum.

0.5335 grm. gave 0.4495 grm. sulphate of lead.

These numbers correspond in 100 parts to

	I.	II.	III.	IV.
Carbon	20.45	15.54	19.21	18.17
Hydrogen	2.06	....	2.11	2.00
Nitrogen	0.79	0.71	0.82	0.72
Oxygen	17.47	....	17.58	17.12
Oxide of Lead	59.23	68.73	60.28	61.99
	100.00		100.00	100.00

After deducting the oxide of lead the relative proportions of the other constituents are expressed by the following numbers:

	I.	II.	III.	IV.
Carbon..........	50.15	49.69	48.36	47.80
Hydrogen.......	5.05	....	5.31	5.26
Nitrogen........	1.93	2.27	2.06	1.89
Oxygen.........	42.87	....	44.27	45.05
	100.00		100.00	100.00

These numbers conduct, as will be seen, to two different formulæ. The numbers of the first two determinations lead to the formula $C_{52}\ H_{31}\ NO_{34}$, those of the last two to the formula $C_{52}\ H_{33}\ NO_{36}$ as a comparison of the numbers found by experiment with those required by the respective formulæ will show:

	Eqs.		Calculated.	Eqs.		Calculated.
Carbon....	52	312	49.60	52	312	48.22
Hydrogen..	31	31	4.92	33	33	5.10
Nitrogen ..	1	14	2.22	1	14	2.16
Oxygen...	34	272	43.26	36	288	44.52
		629	100.00		647	100.00

The first analyses which I made of the lead compounds of indican, and the results of which are recorded in the first part of this paper, led to the formulæ $C_{52}\ H_{35}\ NO_{38}$ and $C_{52}\ H_{37}\ NO_{40}$, but I stated at the same time that neither of these could be considered as the true formula, since the compounds then analysed seemed no longer to contain unchanged indican. However the compounds, the analyses of which have just been given, even after having been completely dried, and decomposed with cold dilute sulphuric acid yielded solutions which, after having been filtered from the sulphate of lead and boiled, deposited flocks consisting almost entirely of indigo-blue and indirubine, products indicating with certainty the presence of unchanged indican. Since indican exhibits a tendency, as I have before observed, to take up successively a number of equivalents of water, it is probable that

of the two formulæ given above, the first, viz., $C_{52}\ H_{31}\ NO_{34}$ approaches nearest to, if it is not a correct representation of its true composition. The formula $C_{52}\ H_{33}\ NO_{36}$ may then represent a mixture of indican with a small quantity of what may be called its hydrate, or it may show the composition of indican in the first stage of its hydration before it has lost the property of yielding indigo-blue by decomposition. As far as regards the explanation of the different processes of decomposition which the substance undergoes, it is of course immaterial which formula is adopted.

Action of Acids on Indican.

In the first part of this memoir I have given a general description of the process of decomposition which indican undergoes by the action of acids and of the products thereby formed. I shall now proceed to give an account of the results obtained in a more minute investigation of this process, performed with larger quantities of material than had previously been at my disposal.

Sulphuric and muriatic acids are not the only acids capable of effecting the decomposition of indican. If to a watery solution of the latter a small quantity of nitric acid be added, the quantity of the acid not being large enough to enable it to exert any oxidising action on the indican, the solution immediately becomes green and turbid, and on standing it deposits flocks of a dark colour, while the surface becomes covered with a blue pellicle. The deposit is found to consist principally of indigo-blue with a little indirubine and a trace of other products of decomposition. The filtered liquid on being boiled becomes muddy and deposits some brown flocks, which contain no indigo-blue. The quantity of indigo-blue formed by the action of nitric acid seems indeed to be comparatively larger than when sulphuric or muriatic acid is employed. It is hardly necessary to add, that if this be really the case, it cannot be ascribed to any oxidising effect produced by the

acid. A watery solution of indican on being mixed with oxalic or tartaric acid and left to stand yields a dark blue or purple deposit, consisting of indigo-blue and indirubine, which when oxalic acid is employed, are remarkably free from other products of decomposition. The liquid filtered from this deposit yields in either case when boiled a few more flocks, and after being filtered, mixed with sulphuric acid and boiled again, it gives an additional quantity. These flocks contain indirubine and indiretine but no indigo-blue. Even acetic acid produces a slight effect on indican. On adding that acid to a watery solution of the latter, the mixture deposits on standing some dark flocks, consisting of indigo-blue and indirubine, but their quantity is trifling.

A more minute examination of this process of decomposition showed that it was more complicated and that the products formed by it were more numerous than I had at first imagined. The products of decomposition which I have observed are of three kinds. The first are insoluble in water and are deposited in the shape of powder or flocks from the acid liquid, the second remain dissolved in the latter, the third are volatile and are obtained by distilling the liquid either whilst the action of the acid is proceeding or after it has ceased. For the purpose of preparing these various products I found it to be unnecessary to obtain indican in a state of absolute purity by successive solution in alcohol, water and ether, for though some indican is always decomposed when its watery solution is evaporated, the substances into which it is thereby converted afford by decomposition with acids, products which do not differ in their nature, but only in their relative proportions from those which are formed, when perfectly pure indican is employed. I therefore contented myself with extracting the dry leaves of the woad plant with cold alcohol, evaporating the extract in the apparatus above described, adding water to the residue and filtering. The solution of indican thus obtained was mixed with a considerable

quantity of sulphuric acid, and the green fatty matter precipitated by the acid was separated by filtration. The action of the acid passed, as I invariably observed, through two distinct stages, and I found it convenient to collect and treat the products formed at each stage of the action separately. The filtered liquid, though clear at first, soon became opalescent and muddy and deposited dark flocks, while the surface became covered with a blue pellicle. After the liquid had stood in the cold for about twenty-four hours, this deposit usually ceased to be formed, and the action then entered on its second stage, which was manifested by the separation from the filtered liquid of a brown powder the quantity of which was much increased by heating. This powder contained little or no indigo-blue, but some indirubine and a large quantity of other products of decomposition. I think it probable that the first deposit owed its origin to the pure indican contained in the solution, while the second was formed from indican that had undergone a change by the action of water. The matter insoluble in water formed by the action of acid having been collected on a filter, the acid liquid was employed for the preparation of the other products of decomposition in a manner to be hereafter described. The portion of the products insoluble in water was also obtained by another method, still more expeditious than the one just described. The leaves of the plant having been finely chopped, boiling water was poured over them, and the mixture having been well stirred the liquid was strained through calico and mixed with sugar of lead. This produced a pale green precipitate which was separated by filtration, and the liquid having been mixed with an excess of sulphuric acid was filtered from the sulphate of lead and heated for some time, when it produced a deposit containing the same products as before. Instead of sulphuric acid I sometimes employed nitric acid, avoiding however in this case the use of heat. More indigo-blue and less of the other pro-

ducts of decomposition seemed to be formed, when nitric acid was used.

In whatever manner the products insoluble in water were obtained, I always adopted the same method of treatment for the purpose of separating them from one another, a method which is indeed essentially the same as that employed by Berzelius for the separation of the constituents of crude indigo. The whole of the acid used in the process having been carefully removed by means of cold water, the mass left on the filter was treated with dilute caustic soda. This dissolved a great portion forming a dark brown opaque liquid, which was filtered from the insoluble matter. The latter was treated again with caustic soda, the action being now assisted by heat, and the process was repeated until nothing more was dissolved. The liquid on being mixed with an excess of muriatic acid let fall a voluminous flocculent precipitate of a brown colour, which after being collected on a filter and washed with water was treated with a boiling mixture of alcohol and ammonia. This sometimes dissolved the whole of it, sometimes only a part. The insoluble portion, when there was any present, had the appearance of a dark brown powder, and consisted of the body to which I have given the name of *Indihumine*. After having been treated repeatedly with alcohol and ammonia, until nothing more was dissolved and then with muriatic acid, and lastly washed with water, it was considered pure. The alcoholic liquid filtered from it was dark brown. On adding to it an excess of acetic acid, an abundant dark brown deposit was formed, consisting of a substance which I had not previously observed, and to which I propose to apply the term *Indifuscine*. It was collected on a filter, washed first with alcohol, then with hot water until all the acetate of ammonia and acetic acid were removed, and lastly agitated with a little cold alcohol, filtered off and dried, when it had the appearance of a dark brown or reddish-brown powder. By repeating the process of solution in alcohol and ammonia and precipitation

with acid, its further purification was effected. The alcoholic liquid filtered from the indifuscine was mixed with an alcoholic solution of acetate of lead, when an additional quantity of the same substance was precipitated in combination with oxide of lead in brown flocks. The filtered liquid, containing an excess of sugar of lead was mixed with ammonia, which gave a brownish-yellow precipitate, consisting chiefly of indiretine in combination with oxide of lead. This precipitate after being filtered off was treated with dilute acetic acid, which removed a considerable quantity of oxide of lead, and after being again filtered off and washed, it was completely decomposed with boiling dilute muriatic acid. The indiretine which was separated collected in the boiling liquid in the form of brown half fused masses, which were separated by filtration while the liquid boiled, washed with boiling water and then treated with a small quantity of cold alcohol. The alcohol acquired a dark brown colour, and after being filtered from a little undissolved indifuscine was evaporated to dryness, when it left the indiretine in the shape of a brittle resinous residue, which was purified by being again dissolved in cold alcohol.

That part of the product of the action of acids insoluble in caustic soda was usually of a dark bluish-purple colour. It was treated with boiling alcohol until nothing more dissolved. The alcoholic liquid, which had a dark brownish-purple colour, was filtered boiling hot from the insoluble portion, consisting chiefly of indigo-blue, and then mixed with ammonia and an alcoholic solution of acetate of lead, which gave a brown precipitate consisting of oxide of lead in combination with indifuscine, and such other products as had not been completely extracted by the caustic soda. After being filtered from this precipitate the liquid appeared of a beautiful purple colour. It was mixed with an excess of acetic acid and distilled or evaporated to about one quarter of its original volume, and then mixed with a large quantity of water, which precipitated the whole of the matter dissolved in it in the shape of dirty

purple flocks. These flocks were collected on a filter, well washed with water and then treated with dilute caustic soda, which generally however only dissolved a minute portion of them. After being again filtered off and well washed they were dried and treated with a small quantity of cold alcohol. The alcohol dissolved a portion forming a solution of a deep reddish-yellow colour, which was filtered and evaporated, when it left a shining resinous substance of the same colour, which as it possesses characteristic properties and a peculiar composition, I shall call *Indifulvine*. By dissolving it in weak spirits of wine it was separated from a little impurity, which remained undissolved in the shape of a brown powder. The matter left undissolved by the cold alcohol consisted chiefly of indirubine. For the purpose of purifying this body I availed myself of the property which it possesses in common with indigo-blue of dissolving in caustic alkalies in the presence of bodies which easily take up oxygen. On treating the mixture containing indirubine with a solution of protoxide of tin in caustic soda and boiling, I obtained a solution, which after being rapidly filtered deposited indirubine on exposure to the air in the shape of a reddish-purple pellicle covering its surface. This pellicle on being broken fell to the bottom in thick flakes and was succeeded by another. As soon as the whole of the indirubine contained in it had been again oxidised and deposited, it was filtered off, well washed with water and dissolved in boiling alcohol. The alcoholic solution which had a beautiful purple colour generally left on evaporation a dark brown amorphous residue consisting of indirubine in as high a state of purity as I have been able to obtain it when formed by the decomposition of indican. A brown powder was left undissolved by the alkaline solution of protoxide of tin, which after being again treated with a fresh quantity of the same solution, in order to dissolve all the indirubine which might be contained in it, was washed with water, then with acid, washed again with

water, dried and treated with cold alcohol. The latter dissolved a second portion of indifulvine, which seemed to have escaped the solvent action of the alcohol in the first instance in consequence of its having been so intimately mixed with and enveloped by particles of indirubine as not to be reached by the alcohol. The alcohol still left undissolved a quantity of brown powder, which did not seem to be any peculiar substance but an intimate mixture of indifulvine and indirubine. The indigo-blue left undissolved by the boiling alcohol was purified by treating it according to Fritzsche's method with a warm solution of grape sugar in alcohol to which caustic soda was added, and allowing the mixture to stand in a warm place until the indigo-blue was dissolved. The yellow solution having been drawn off with a syphon and allowed to stand exposed to the air became first red, then purple, and then deposited the indigo-blue in the shape of small crystalline scales, which were collected on a filter and washed first with alcohol, afterwards with boiling water, then digested with muriatic acid, well washed with water and dried.

The bodies insoluble in water formed by the action of acids on indican are therefore six in number. I shall now give an account of their properties and composition.

Indigo-blue.

The indigo-blue obtained by this process has all the properties usually ascribed to that substance. It is insoluble in alkaline liquids, but dissolves easily when a deoxidising substance, such as a salt of protoxide of tin or protoxide of iron, or grape sugar is added at the same time, the solution exhibiting the usual appearance of an indigo vat, such as the yellow colour, and the blue pellicle on the surface. It is only slightly soluble in boiling alcohol, to which it communicates a blue tinge, but easily and completely soluble in concentrated sulphuric acid, forming a blue solution from which nothing is precipitated on the addition of water. By the action of

boiling nitric acid it yields indigotic acid, and when treated with a strong boiling solution of caustic soda it is converted into an acid, having the properties of anthranilic acid. Its identity with indigo-blue is however placed beyond doubt by its analysis, which yielded the following results:—

I. 0.3365 grm. dried at 100° C. and burnt with oxide of copper and chlorate of potash gave 0.8955 grm. carbonic acid and 0.1305 water.

0.5175 grm. burnt with soda-lime gave 0.3775 grm. platinum.*

II. 0.3605 grm. gave 0.9605 grm. carbonic acid and 0.1350 water.

0.5230 grm. gave 46 CC. of moist nitrogen at a temperature of 12° C. and a pressure of 736.8$^{mm.}$ equivalent to 42.7 CC. of dry nitrogen at 0° C. and a pressure of 760$^{mm.}$ or 0.0534 grm.

Hence was deduced the following composition:—

	Eqs.		Calculated.	I.	II.
Carbon......	16	96	73.28	72.57	72.66
Hydrogen...	5	5	3.81	4.30	4.16
Nitrogen....	1	14	10.68	10.36	10.22
Oxygen.....	2	16	12.23	12.77	12.96
		131	100.00	100.00	100.00†

* The double chloride which yielded this amount of platinum was washed according to Hofmann's directions with ether, to which a little alcohol was added instead of with the usual mixture of alcohol and ether. It weighed 0.9095 grm., which if it had consisted of the double chloride of platinum and ammonium alone, would have corresponded to 0.0571 grm. of nitrogen or 11.03 per cent. The apparent excess arose without doubt from the presence of aniline.

† The analyses above given lead to a composition more nearly approaching the theoretical one than any previously on record, with the exception of those given by Laurent (Ann. de Chim. et de Phys. Ser. III., T. 3, p. 372), which were made with sublimed indigo-blue. To the use of the latter for this purpose, however, objections may be raised on account of the difficulty of separating it from particles of carbon and traces of oily and resinous matters formed by the sublimation. Dumas in his last memoir on the composition of indigo-blue (Ann. de. Chim. et de Phys. Ser. III,. T. 2, p. 207) proved that the excess of carbon in the previous analyses of the substance was only apparent, being caused by the admixture of a little sulphur derived from the sulphate of iron which is generally used for its purification. Having carefully removed this

INDIRUBINE.

This substance when obtained by the process above described usually appears in the form of a dark brown amorphous mass. On one occasion, when very pure indican had been employed in its preparation, it was deposited from the boiling alcoholic solution on cooling in long crystalline needles, which were red by transmitted light. The alcoholic solution has a fine purple colour. It is perfectly insoluble in alkaline liquids, but if it be treated with a boiling solution of caustic soda, to which some deoxidising substance, such as protochloride of tin or grape sugar is added, it dissolves with ease, just as indigo-blue does under the same circumstances, forming a solution from which it is again deposited in purple flakes by the action of the atmospheric oxygen. It dissolves in concentrated sulphuric acid in the cold, forming a purple solution, which on the addition of water gives a dark precipitate, the supernatant liquid remaining of a fine purple colour. It is decomposed by boiling nitric acid. On being heated

sulphur, he obtained in three analyses 72.90, 72.84, and 72.97 per cent. of carbon, which correspond apparently with the theoretical composition. These amounts are, however, calculated according to the old atomic weight of carbon. If corrected in accordance with the new atomic weight of carbon, which was established by Dumas a short time previously, they become respectively 71.89, 75.77, and 71.92, the great excess in the second determination being probably due to some misprint. On analysing some specimens of the indigo-blue remaining from his previous investigation, which he himself had proved to be impure, and calculating the results according to the new atomic weight of carbon, Dumas obtained in four analyses 73.3, 73.5, 72.7, and 73.3 per cent. of carbon. The coincidence between these and the previous analyses is of course only apparent. I have myself always found a deficiency in the amount of carbon, unless care was taken to wash the precipitated indigo-blue for a considerable time. I ascribe this circumstance to the indigo-blue like all porous bodies combining with certain substances and removing them from their solutions in consequence of an attraction of surface exerted by it. If, for instance, grape sugar is employed in its purification, a certain quantity of it is carried down by the indigo-blue and can only be removed by continuous washing with hot water, followed by treatment with muriatic acid and renewed washing with water. Each portion of water is found to leave on evaporation a small quantity of syrup, and this does not cease until the washing has been ontinued for several days.

between two watch-glasses, it produces on the upper glass a sublimate consisting of beautiful purple needles which dissolve in boiling alcohol forming a fine purple solution which on cooling deposits crystalline needles. This sublimate seems to consist, not of any product of decomposition formed by heat, but of the substance itself, which when freed from all impurities possesses the property of crystallising.

The quantity of indirubine which I obtained, even when operating on large quantities of indican, was so exceedingly small, that I was unable to apply any means for effecting its further purification.

I was however enabled by chance to procure from another source a sufficient quantity of the substance for an examination of its properties and composition. Some time before commencing my investigation of the woad plant I had obtained from India a quantity of the dried leaves of the *Indigofera tinctoria* for the purpose of ascertaining the state in which the colouring matter is contained in them. Though the leaves reached me as soon as possible after having been gathered and dried, their examination led to no definite results, the process of fermentation by which the colouring matter is formed having probably been already completed, and I therefore laid them aside. Their peculiar greenish-purple colour and the glaucous appearance of their surface which resembled that of glazed green tea, showed however that they must contain, ready formed, some peculiar species of colouring matter. I was therefore induçed to examine them again, and this examination led to the conclusion that their colour was caused by a thin coating of a substance, which was, there could be little doubt, identical with indirubine. This substance was isolated by the following process.

Having prepared a liquid containing protochloride of tin dissolved in an excess of caustic soda, the leaves were immersed in it while boiling. The boiling was continued until the leaves had lost their purple tinge and become pale green.

The green muddy liquid was then strained as quickly as possible through canvas, and left to stand exposed to the air in a shallow vessel. The surface of the liquid soon became covered with a purple pellicle, which was carefully skimmed off and was succeeded by another, which was in its turn removed, the process being repeated as long as anything formed on the surface. The purple matter was then dissolved a second time in an alkaline solution of protoxide of tin and the solution was again left exposed to the atmosphere. The pellicle which was formed by the action of the oxygen was removed this time by means of blotting paper, to which it adhered without much of the liquid underneath being removed with it. The substance was separated from the paper by agitation in water, collected on a filter, treated with boiling caustic soda to dissolve a little adhering fatty matter, filtered off again, washed with acid, then with water, and lastly dissolved in boiling alcohol. The alcohol acquired a splendid purple colour and on cooling deposited a quantity of crystalline needles, consisting, as I believe, of indirubine in a state of purity. When thus prepared it is found to have the following properties.

It crystallises from its alcoholic solution in small needles forming when dry a silky mass of a colour between purple and chocolate, which on being rubbed with a hard body shows a slight metallic lustre, resembling that of bronze. When heated on platinum foil it emits red vapours, then melts and burns with a yellow smoky flame leaving some charcoal. When carefully heated between two watch-glasses it gives a yellowish-red vapour, resembling that of bromine, which condenses on the upper glass in the form of beautiful long crystalline needles. These needles are plum or garnet-coloured, they possess a somewhat metallic lustre, which is however much inferior to that of sublimed indigo-blue, and seem to consist simply of the original substance, which has been volatilised without change. When the process of sub-

limation is carefully conducted only a trace of carbonaceous residue is left. It dissolves completely in concentrated sulphuric acid in the cold, forming a solution of a beautiful purple colour. This solution when heated does not become black, but on the contrary rather paler and evolves only a trace of sulphurous acid. When mixed with water it gives no precipitate and retains its fine purple colour, which does not disappear or become weakened when the acid is neutralised with carbonate of soda, but soon vanishes entirely when an excess of caustic soda or ammonia is added. The solution in sulphuric acid after dilution with water imparts a fine purple colour to cotton, wool, and silk. When treated with nitric acid of ordinary strength indirubine begins to dissolve even in the cold and to a greater extent on the application of heat, forming a purple solution, which on being further heated becomes red and on boiling yellow. The whole of the substance is dissolved without leaving any resinous residue, such is always left when indigo-blue is treated with nitric acid, forming a clear yellow solution. This solution leaves on evaporation a residue which dissolves only partially in hot water. A brown resinous substance is left undissolved by the latter, and the liquid filtered from this is bright yellow and very bitter and yields when mixed with carbonate of potash and evaporated, crystals, apparently of picrate of potash, which detonate when heated. Very dilute nitric acid also decomposes and dissolves it on boiling, but its decomposition is effected with far more difficulty than that of indigo-blue by the same means. In like manner a boiling solution of bichromate of potash, mixed with sulphuric acid, which easily decomposes indigo-blue, seems to have very little effect on it even when the boiling is continued for a considerable time. When suspended in water and exposed to the action of a stream of chlorine gas it loses its colour very slowly and is changed into a brown resinous substance containing chlorine which melts

in boiling water and is easily soluble in alcohol, but does not crystallise when the solution is evaporated. Like the indirubine from indican it is quite insoluble in alkaline liquids, but dissolves easily when a deoxidising agent, such as grape sugar or a protosalt of iron or tin, is added at the same time. If it be treated for instance with a solution of protoxide of tin in an excess of caustic soda it dissolves rapidly, forming a yellow solution, the surface of which on exposure to the air instantly becomes covered with a film of regenerated indirubine, the appearance being exactly like that of an indigo vat, except that the film floating on the surface is purple instead of blue. If a piece of calico be dipped into the solution and then exposed to the air it acquires a purple colour, which is not removed either by acids or soap. This colour has no great intensity, but by working on a larger scale it is probable that shades of purple equal in depth to those produced by indigo-blue might be obtained. When the solution is mixed with an excess of muriatic acid it gives a dirty yellow precipitate, which after filtration and exposure to the air slowly becomes purple. By long continued exposure of the solution to the atmosphere the whole of the indirubine dissolved in it is again deposited as a purple mass, which is sometimes found to consist of small crystalline needles. When heated in a tube with soda-lime the substance emits fumes having a smell like that of benzol and an alkaline reaction, which condense on the colder parts of the tube to a sublimate, consisting partly of oil and partly of crystalline needles. It is not precipitated from its alcoholic solution by acetate of lead, even when ammonia is added at the same time. These reactions seem to me to prove the identity of this body with the indirubine from indican, which if it could be entirely freed from all impurities would no doubt exhibit the same property of crystallising and of volatilising without residue. *

* When dry woad leaves are extracted with cold alcohol, the sides of the glass vessel containing the extract generally become covered with patches of

The behaviour of indirubine towards concentrated sulphuric acid and towards alkaline solutions of deoxidising substances so much resembles that of indigo-blue towards the same reagents as to lead one to expect a great similarity in the composition of the two bodies, even if the fact of their being formed from the same parent substance by the same process of decomposition were unknown. The quantity of pure indirubine, which I obtained from the leaves of the Indigofera, was only sufficient for a general examination of its properties and for one analysis, which showed however, if it be permitted to draw a positive conclusion from one determination, that it has exactly the same elementary composition as indigo-blue, that the two substances are isomeric. The following are the numbers yielded by the analysis:—

0.3185 grm. dried at 100°C. and burnt with oxide of copper and chlorate of potash gave 0.8500 grm. carbonic acid and 0.1195 water.

0.2021 grm. gave 49.3 CC. of nitrogen at a temperature of 10.5° C. and a pressure of 269.5$^{mm.}$ equivalent to 16.81 CC. at 0° C. and a pressure of 760$^{mm.}$ or 0.2122 grm.*

In 100 parts in contained therefore

Carbon	72.78
Hydrogen	4.16
Nitrogen	10.50
Oxygen	12.56
	100.00

Indifulvine.

This substance is obtained on the evaporation of its alcoholic solution in the form of a deep reddish-yellow, transparent, amorphous resin, which when dry is brittle and may easily be reduced to powder. It is perfectly insoluble in caustic alkalies,

small red crystals, which seem to consist of pure indirubine. They are insoluble in caustic alkalies, but soluble in boiling alcohol, the solution depositing on cooling, crystals exactly like those above described.

* I owe this determination to Professor Frankland, who had the kindness to perform it according to his own method of analysis.

a property by which it may be at once distinguished from indiretine, which it resembles in its outward appearance. Even when grape sugar or protochloride of tin is added to the alkaline liquids not a trace of it dissolves even on boiling, and in this respect it differs widely from indirubine. When it is treated with strong caustic soda lye only a trace of ammonia is given off, but on heating the dry substance with soda-lime there is a very perceptible evolution of ammonia. When heated on platinum it melts and then burns with a bright flame, leaving much charcoal which burns away with difficulty. On being heated in a tube it melts and gives off fumes having a strong smell resembling that of crude indigo when heated. These fumes condense on the colder parts of the tube to a brown oil which on cooling becomes almost solid without exhibiting a trace of anything crystalline. It dissolves in concentrated sulphuric acid in the cold forming a solution of a greenish-brown colour, which when heated becomes black and disengages sulphurous acid. It is not much affected by nitric acid of ordinary strength even on boiling, but fuming nitric acid dissolves it readily, even in the cold, giving a dark reddish-yellow solution, which on the addition of water deposits orange-coloured flocks. If the solution in fuming nitric acid be boiled it gives off nitrous acid, and on evaporation it leaves a reddish-yellow resinous mass, the greatest part of which on being treated with boiling water remains undissolved in the shape of a yellowish-red resin, resembling indifulvine itself in appearance, but differing from it in being easily soluble in alkaline liquids and soluble with difficulty in boiling alcohol. The watery liquid filtered from this resin yields on evaporation white needle-shaped crystals which are not oxalic acid. A boiling solution of bichromate of potash to which sulphuric acid is added decomposes indifulvine very slowly, the solution becoming green from the reduction of the chromic acid. Chlorine converts indifulvine when suspended in water into a body which does

not much differ from it in appearance but is soluble in alkalies. Indifulvine is not precipitated from its alcoholic solution by acetate of lead even on the addition of ammonia, as indeed might be inferred from its method of preparation.

Notwithstanding that I worked with tolerable large quantities of the mixed products of decomposition of indican I obtained only on two occasions, a sufficient quantity of pure indifulvine for analysis. The composition on these two occasions was not the same, so that, if the substance was in each case pure, there are, strictly speaking, two bodies having the general properties of indifulvine. Nevertheless the formulæ of the two bodies stand in a definite relation to one another and to that of indican, so that in either case the formation of the substance may easily be explained.

I. 0.3695 grm. dried at 100° C. gave 0.9945 grm. carbonic acid and 0.1795 water.

0.3605 grm. gave 0.4665 grm. chloride of platinum and ammonium.

These numbers lead to the following composition :—

	Eqs.		Calculated.	Found.
Carbon........	22	132	73.33	73.40
Hydrogen.....	10	10	5.55	5.39
Nitrogen......	1	14	7.77	8.12
Oxygen.......	3	24	13.35	13.09
		180	100.00	100.00

The second analysis afforded the following data :—

II. 0.3125 grm. gave 0.8975 grm. carbonic acid and 0.1635 water.

0.3400 grm. gave 0.4635 grm. chloride of platinum and ammonium.

Hence may be deduced the following composition :—

	Eqs.		Calculated.	Found.
Carbon........	44	264	78.80	78.32
Hydrogen.....	19	19	5.67	5.81
Nitrogen......	2	28	8.35	8.56
Oxygen.......	3	24	7.18	7.31
		335	100.00	100.00

If the first formula be doubled it gives $C_{44}\ H_{20}\ N_2\ O_6$, and if from this be deducted 1 equivalent of water and 2 equivalents of oxygen it gives the second formula $C_{44}\ H_{19}\ N_2\ O_3$.

For the sake of distinction I think it may be of advantage to apply to the first of these modifications of indifulvine, the term *a* indifulvine and to the second that of *b* indifulvine. The manner in which these bodies are derived from indican can only be understood after all the products of decomposition have been treated of.

Indihumine.

This substance has the appearance of a sepia-brown powder, which is insoluble in water and alcohol, but soluble in alkaline liquids forming brown solutions, from which it is re-precipitated by acids in brown flocks. When heated on platinum it burns without melting leaving some charcoal which easily burns away. Boiling nitric acid decomposes it easily, forming a yellow solution which on evaporation leaves an orange-coloured residue insoluble in water. Indihumine is not always formed in any great quantity in the decomposition of indican by acids. Sometimes indeed it cannot be detected among the products of decomposition, and usually it is present only in minute quantities. What are the circumstances which determine its formation in particular cases I am unable to say. On the only occasion on which I obtained a sufficient quantity for analysis it was procured from an alcoholic extract of woad by evaporating, adding water to the residue and filtering, then adding sulphuric acid to the watery solution containing indican, filtering again, allowing the solution to stand for twenty-four hours, filtering off the indigo-blue and other products which had separated, boiling the liquid, collecting the brown powder which was deposited during the ebullition on a filter, washing it with water and then treating it with a boiling mixture of alcohol and ammonia until nothing more dissolved. The insoluble

residue consisting of indihumine was analysed, when the following numbers were obtained:—

0.3065 grm. dried at 100° C. gave 0.7065 grm. carbonic acid and 0.1300 water.

0.3285 grm. gave 0.3765 grm. chloride of platinum and ammonium.

From these numbers it may be inferred that the composition is as follows:—

	Eqs.		Calculated.	Found.
Carbon	20	120	62.82	62.86
Hydrogen	9	9	4.71	4.71
Nitrogen	1	14	7.33	7.19
Oxygen	6	48	25.14	25.24
		191	100.00	100.00

INDIFUSCINE.

This body so much resembles the preceding in its outward appearance and most of its properties, that the two might easily be confounded. Indifuscine is always obtained in the shape of a dark brown powder, exhibiting sometimes a reddish tinge. It is insoluble in boiling water and only slightly soluble in boiling alcohol, the solution being light brown and depositing a great part of the substance on cooling in brown flocks. It is easily soluble in a mixture of alcohol and ammonia. The solution is dark brown and opaque, and when it is mixed with an excess of muriatic or acetic acid the greatest part of the indifuscine is deposited in the form of a brown powder, while the supernatant liquid retains a brown colour, which is rather darker than that of the solution of the substance itself in boiling alcohol. It is also soluble in watery solutions of caustic and carbonated alkalies, forming brown solutions, from which it is precipitated again by acids in brown flocks. The ammoniacal solution gives brown precipitates with salts of baryta, lime, magnesia, alumina, iron, zinc, copper, lead, mercury, and silver, the whole of the in-

difuscine being precipitated in combination with the respective bases. When indifuscine is heated in a platinum crucible the whole mass begins to heave and is kept in a state of agitation for a few moments, in consequence probably of an evolution of gas at the points of contact with the metal, whereupon it burns but without melting and leaves a considerable quantity of charcoal, which burns away with difficulty without leaving any ash. When heated in a tube it gives fumes having a smell like that of burning turf besides a little oily sublimate, unmixed with anything crystalline. Concentrated sulphuric acid dissolves indifuscine forming a brown solution, which on being heated evolves sulphurous acid. A boiling solution of bichromate of potash to which sulphuric acid is added dissolves and decomposes it rapidly with an evolution of gas, the chromic acid being reduced to oxide of chromium. On being treated with boiling dilute nitric acid indifuscine is decomposed with a disengagement of nitrous acid, giving a yellow liquid which on evaporation yields crystals of oxalic acid. The mother liquor of these crystals on being neutralised with carbonate of potash and evaporated gives brownish-yellow crystals, which detonate when heated and give with acetate of lead, nitrate of silver, and sulphate of iron, reactions showing them to consist of picrate of potash. When finely powdered indifuscine is suspended in water and subjected to the action of chlorine it is converted into a yellow flocculent substance containing chlorine, which is insoluble in boiling water, but dissolves easily in boiling alcohol forming a brown solution, which on spontaneous evaporation leaves a light brown amorphous residue.

When the indican submitted to decomposition with acids has not been purified, the quantity of indifuscine formed far exceeds that of the other products of decomposition, which with the exception of indigo-blue are always produced in comparatively small quantities. In this case a great part of the indifuscine owes its origin to the action of the acid on a body

formed by the influence of water and oxygen on indican. Nevertheless, even when perfectly pure indican is employed, a certain quantity of indifuscine is always produced, especially if the quantity of material used is considerable.

When submitted to analysis indifuscine prepared on different occasions is found to exhibit considerable variation in its composition. The analyses, the results of which I am about to give, were made with specimens, derived from different sources, which notwithstanding the difference in their composition showed no difference in their properties.

I. 0.3135 grm. indifuscine, obtained from the deposit formed on mixing a watery solution of indican with sulphuric acid and allowing the mixture to stand in the cold, dried at 100° C. and burnt with oxide of copper and chlorate of potash, gave 0.6830 grm. carbonic acid and 0.1305 water.

II. 0.3930 grm. obtained by adding sulphuric acid to a watery solution of indican, allowing the mixture to stand for some time in the cold, filtering, and then employing the deposit produced on heating the filtered liquid, gave 0.8720 grm. carbonic acid and 0.1625 water.

0.5675 grm. gave 0.5230 grm. chloride of platinum and ammonium.

III. 0.3435 grm. derived from the deposit formed on mixing a decoction of woad leaves with muriatic acid and boiling, gave 0.7635 grm. carbonic acid and 0.1510 water.

0.4550 grm. gave 0.4365 grm. chloride of platinum and ammonium.

IV. 0.3675 grm. obtained by extracting finely chopped woad leaves with warm water, adding sugar of lead to the extract, filtering from the green precipitate, removing the excess of lead with sulphuric acid, filtering, adding more acid, and treating the flocculent deposit which was formed in the usual manner, gave 0.8950 grm. carbonic acid and 0.1635 water.

0.5715 grm. gave 0.5855 grm. chloride of platinum and ammonium.

V. 0.3640 grm. prepared in a similar manner to the last, gave 0.9020 grm. carbonic acid and 0.1640 water.

0.4470 grm. gave 0.5070 grm. chloride of platinum and ammonium.

These numbers correspond in 100 parts to—

	I.	II.	III.	IV.	V.
Carbon......	59.41	60.51	60.61	66.41	67.58
Hydrogen...	4.62	4.59	4.88	4.94	5.00
Nitrogen..........		5.78	6.02	6.43	7.12
Oxygen...........		29.12	28.49	22.22	20.03
		100.00	100.00	100.00	100.00

In passing the eye along these numbers it will be seen that the amount of oxygen decreases progressively from II. to V., while that of the other constituents increases. These two analyses represent in my opinion the composition of the two extreme members of a series, the intermediate members of which consist of mixtures or compounds of the two. A number of other analyses which I made gave results which can only be explained on the supposition that there are two bodies having the same general properties which I have ascribed to indifuscine, but a different composition. Of the above analyses the two first agree tolerably well with the formula $C_{24} H_{10} N O_9$, whereas the last leads to the formula $C_{22} H_{10} N O_5$, as the following calculation will show:—

	Eqs.		Calculated.	Eqs.		Calculated.
Carbon....	24	144	60.00	22	132	67.34
Hydrogen..	10	10	4.16	10	10	5.10
Nitrogen ..	1	14	5.83	1	14	7.14
Oxygen...	9	72	30.01	5	40	20.42
		240	100.00		196	100.00

It will be seen that the two formulæ differ from one another by 2 equivalents of carbonic acid, and hence the name of *Indifuscone* might not be inappropriate for the body whose composition is expressed by the second formula $C_{22} H_{10} N O_5$.

Though this is the only way in which I am able to explain these discrepancies, still I failed in all my efforts to separate any specimen of the substance having an intermediate composition into two distinct constituents, as every such specimen behaved towards all reagents as if it were one single substance. By treating however a specimen of this kind with a boiling solution of caustic soda for a length of time, the percentage of carbon was increased by about 2.5, showing that the body whose formula is $C_{24} H_{10} NO_9$ has a tendency to lose carbonic acid and be converted into the one whose composition is expressed by the formula $C_{22} H_{10} NO_5$. The substance used in this experiment was that employed for the analysis No. III. It was dissolved in caustic soda, the solution was boiled for some time and then mixed with an excess of muriatic acid. The precipitate produced by the acid was dissolved in alcohol and ammonia, and the solution having been mixed with an excess of acid deposited a brown powder, which after being collected on a filter, washed and dried was analysed, when it was found to contain 63.22 per cent. of carbon.

INDIRETINE.

This body, the most striking properties of which have been already mentioned in the first part of this paper, appears on evaporation of its alcoholic solution in the form of a dark brown, shining resin, which is transparent only in very thin layers. It resembles indifulvine in appearance but is distinguished from the latter by its being easily soluble in all alkaline liquids. When heated on platinum foil it melts, swells up very much and burns with a yellow smoky flame, leaving some charcoal which slowly burns away. When heated in a tube it swells up and gives strong smelling fumes together with an oily sublimate, resembling that obtained from indifulvine, which when cool becomes half solid. Con-

centrated sulphuric acid dissolves it in the cold, forming a brown solution, which when boiled becomes black and disengages sulphurous acid. Boiling nitric acid decomposes it with an evolution of nitrous acid, giving a yellow solution, which on evaporation leaves a brown residue consisting of a resinous substance insoluble in water and a little picric acid. When it is treated with boiling caustic soda lye only a trace of ammonia is given off, but when heated with soda-lime there is a much stronger evolution of ammonia. A boiling solution of bichromate of potash to which sulphuric acid has been added slowly decomposes it with an evolution of gas while the liquid becomes green. The solution in ammonia is brown and gives brown precipitates with the chlorides of barium and calcium and with nitrate of silver, while the liquid in each case becomes colourless. The alcoholic solution gives with acetate of lead a brown precipitate, which dissolves entirely on the addition of acetic acid, and with acetate of copper it gives a slight brown precipitate, the filtered liquid being still dark brown.

In the first part of this paper I have given for indiretine the formula $C_{38}\ H_{20}\ NO_{13}$. The analyses, which I have made with fresh preparations of this body, lead to the formula $C_{36}\ H_{17}\ NO_{10}$, as will be seen from the following details.

I. 0.3955 grm. dried at 100° C. and burnt with oxide of copper and chlorate of potash gave 0.9565 grm. carbonic acid and 0.1995 water.

0.5215 grm. burnt with soda-lime gave 0.1400 grm. platinum.

II. 0.4250 grm. of the same preparation heated to 190° C. and then kept for several hours at 100° C. gave 1.0300 grm. carbonic acid and 0.2090 water.

0.5065 grm. gave 0.1370 grm. platinum.

III. 0.4210 grm. of a different preparation gave 1.0200 grm. carbonic acid and 0.2140 water.

The theoretical composition as compared with that derived from these numbers is as follows:—

	Eqs.		Calculated.	I.	II.	III.
Carbon....	36	216	66.05	65.96	66.09	66.07
Hydrogen..	17	17	5.19	5.60	5.46	5.64
Nitrogen ..	1	14	4.28	3.81	3.84	....
Oxygen...	10	80	24.48	24.63	24.61	....
		327	100.00	100.00	100.00	

I think it is improbable that the discrepancy between the two formulæ, which differ from one another merely by 3 HO, proceeds from any impurity in either case, or that it is to be attributed to the substance having been more carefully dried at one time than at another. Indiretine appears to furnish one of those instances, of which I have met with several during the investigation of this series, of a body exhibiting when prepared on different occasions the same properties but having at one time a composition differing by the elements of water from that which it has at others.

Having described the several products of decomposition formed by the action of acids on indican, which are insoluble in water, I shall now proceed to the consideration of those which are soluble in water. In order to obtain these I found it advisable to employ sulphuric acid for the decomposition of the indican. After the process was completed the insoluble matter deposited was separated by filtration, the sulphuric acid was removed by means of carbonate of lead, and the liquid having been filtered, sulphuretted hydrogen was passed through it in order to precipitate a little lead contained in it, and after being again filtered it was evaporated by means of a current of air in the same apparatus as that employed in the evaporation of solutions of indican. After the evaporation was completed there was left a light brown syrup, which on being treated with alcohol was usually entirely dissolved. The alcoholic solution was filtered if necessary, and then

mixed with about twice its volume of ether, which immediately turned it milky and produced a deposit consisting of a brown syrup. This syrup was allowed to settle and the whole was left to stand for twenty-four hours. The surface of the syrup and the sides of the glass vessel were then found to be covered with a quantity of small, almost white crystals. These crystals are the same as those referred to above as being obtained in the preparation of indican, when ether is added to the alcoholic solution of the latter. I was at first inclined to suppose that they consisted of a substance which was contained as such in the plant, but I soon discovered that they were a product of decomposition of indican, as they were also obtained from perfectly pure indican, which had been prepared by successive solution in alcohol, water and ether, in the last of which the crystals are insoluble. Indeed no product of decomposition of indican seems to be so easily formed as this. By shaking the liquid from which they were deposited the crystals were easily detached from the sides of the vessel and the surface of the syrup. They were collected on a filter, washed with ether, and then pressed between folds of blotting-paper in order to absorb any of the syrup which might be mixed with them. They were then dissolved in boiling water, and the solution having been decolorised with animal charcoal was filtered and evaporated, when it left a crystalline mass, which was again pressed between blotting-paper and dissolved in a small quantity of boiling alcohol. The alcoholic solution on cooling deposited a mass of small crystals, which had the properties and composition of

LEUCINE.

It crystallised from the alcoholic solution in small flat tables having a pearly lustre, which repelled cold water like a fatty acid but were readily soluble in boiling water. It was insoluble

in ether. When heated in a tube it was completely volatilised without melting, forming a sublimate on the colder parts of the tube in the form of a light mass like cotton. It was easily soluble even in the cold in sulphuric, muriatic and nitric acids. The solution in nitric acid gave off no nitrous fumes on being boiled and left on evaporation a colourless syrup, which on standing was changed into a crystalline mass. The solution in muriatic acid left on evaporation a crystalline residue. It was easily soluble in caustic soda, and the solution evolved no ammonia on being boiled, but when the dry substance was heated with soda-lime it gave off a strong smell of ammonia accompanied by a peculiar penetrating odour. The watery solution was neutral to test paper and had no perceptible taste. When mixed with freshly precipitated oxide of copper and boiled, the watery solution became sky-blue; the filtered liquid gave no precipitate with caustic soda, and on being evaporated left a residue consisting of bright blue crystals. The watery solution gave no precipitate with nitrate of silver, but on the addition of a little ammonia there was deposited almost immediately a quantity of small crystalline scales, which blackened slightly on exposure to the light, and were not easily soluble in an excess of ammonia. The watery solution gave no precipitate with acetate of lead, and even on adding ammonia there was only a slight precipitate, but on allowing the ammoniacal liquid to stand for some hours there was formed a crystalline mass of a pearly lustre consisting of needles arranged in star-shaped masses.

The analysis of the substance gave the following results:—

0.3430 grm. dried at 100° C. and burnt with chromate of lead gave 0.6820 grm. carbonic acid and 0.3125 water.

0.2550 grm. gave 0.4125 grm. chloride of platinum and ammonium.

The composition in 100 parts agrees tolerably well with that required by the formula of leucine $C_{12}\ H_{13}\ NO_4$, as will

be seen from the following comparison of the calculated composition with that found by experiment:—

	Eqs.		Calculated.	Found.
Carbon	12	72	54.96	54.22
Hydrogen	13	13	9.92	10.12
Nitrogen	1	14	10.68	10.16
Oxygen	4	32	24.44	25.50
		131	100.00	100.00

However strange the fact of leucine, a substance hitherto supposed to be a product of decomposition peculiar to animal matters, being obtained from the decomposition of a vegetable substance, may have appeared at a former period, it will no longer excite surprise at the present time, when so many different bodies have been found to be common to both classes of organisms. It is a fact however which seems to imply some connection, hitherto unsuspected, between leucine and indigo-blue.

The brown syrup precipitated together with leucine by the addition of ether to the alcoholic solution consisted chiefly of the peculiar kind of sugar produced by the decomposition of indican, and to which as having a composition differing from that of most other species of sugar I propose to give the name of

Indiglucine.

In order to purify it, the brown syrup after the crystals of leucine had been separated by decantation, was dissolved in water and acetate of lead was added to the solution. A slight precipitate was thereby produced, which was separated by filtration, and on adding ammonia to the liquid, a bulky yellowish precipitate fell, consisting chiefly of the lead compound of indiglucine. This was filtered off, completely washed with water and decomposed with sulphuretted hydrogen. The filtered liquid was agitated with animal charcoal until it had

quite lost the yellowish tinge which it showed at first and until a portion of it on being mixed with acetate of lead and ammonia gave a perfectly white precipitate. It was then filtered again and evaporated either in the apparatus above described by means of a current of air or over sulphuric acid. The syrup left after evaporation was dissolved in alcohol and the solution was mixed with twice its volume of ether, when the indiglucine was precipitated as a pale yellow syrup, having a sweetish taste.

To the description formerly given of this substance I have only a few particulars to add. Baryta water gives no precipitate in the watery solution, but on adding alcohol a slight flocculent yellow precipitate is produced. The watery solution after being mixed with milk of lime and filtered is found to have become strongly alkaline and on being boiled becomes quite thick in consequence of the separation of a bulky yellow mass of flocks, which on the liquid cooling is completely redissolved forming a clear yellow solution as before, an experiment which may be repeated any number of times. The solution of the lime compound when mixed with an excess of alcohol gives a bulky yellowish precipitate, after which the liquid appears almost colourless. When treated with boiling nitric acid indiglucine is decomposed and yields oxalic acid. When a watery solution of indiglucine is mixed with yeast and left to stand in a warm place no disengagement of gas is observed nor is any sign of fermentation taking place manifested. After some days however the solution begins to acquire a strongly acid taste and reaction, showing that it has entered into a state of acetous fermentation without having passed through the intermediate stage of the alcoholic fermentation.

The new analyses which I have made of the lead compound confirm the conclusion at which I arrived at an early period of the investigation, viz. that when in combination with oxide

of lead the composition of indiglucine is expressed by the formula $C_{12}\ H_9\ O_{11}$, and that hence its formula when in an uncombined state is probably $C_{12}\ H_{10}\ O_{12}$.

An analysis of the lead compound, prepared by adding acetate of lead and ammonia to a watery solution of indiglucine, filtering, washing and drying in vacuo, gave the following results:—

0.5640 grm. burnt with chromate of lead gave 0.2430 grm. carbonic acid and 0.0845 water.

0.2495 grm. gave 0.2445 grm. sulphate of lead.

These numbers lead like those of the former analyses to the formula $C_{12}\ H_9\ O_{11} + 4\ Pb\ O$, as will be seen by comparing the numbers required by theory with those deduced from the analysis.

	Eqs.		Calculated.	Found.
Carbon	12	72	11.69	11.75
Hydrogen	9	9	1.46	1.66
Oxygen	11	88	14.30	14.49
Oxide of Lead	4	446.8	72.55	72.10
		615.8	100.00	100.00

There still remain some products of the action of acids on indican to be treated of. These products are volatile. In order to ascertain their nature I took a solution of indican, mixed it with sulphuric acid and boiled it in a retort, the tube of which passed through a cork into a receiver from which a tube led into a bottle with lime water, the joinings being all air-tight. After the liquid had entered into a state of ebullition and the air had been expelled from the apparatus, bubbles of gas were seen now and then to pass through the lime water, which became milky and deposited a quantity of carbonate of lime. After a great part of the solution had been distilled, the receiver was removed and the liquid contained in it, which was yellowish and had an acid reaction, was mixed

with an excess of carbonate of soda and evaporated to dryness. The saline residue was mixed with an excess of dilute sulphuric acid and the liquid was distilled. The distillate was now colourless. It contained formic acid, for after being neutralised and mixed with nitrate of silver, metallic silver was soon deposited even in the cold. The whole of it was boiled with carbonate of lead, and the filtered liquid was evaporated, when it yielded some shining crystalline needles surrounded by a thick syrup. By means of a little cold water the syrup was removed, the needles being left undissolved. The latter had the properties of formiate of lead.

0.3450 grm. of these needles dried at 100° C. and heated with sulphuric acid gave 0.3505 grm. sulphate of lead, equivalent to 0.2579 oxide of lead or 74.75 per cent. In 100 parts of formiate of lead there are contained by calculation 75.11 parts of oxide of lead.

The liquid poured off from these crystals was mixed with an excess of sulphuric acid, filtered from the sulphate of lead and distilled. The distilled liquid was boiled with peroxide of mercury, in order to decompose any formic acid which it might contain, and filtered, and after sulphuretted hydrogen had been passed through it, it was again filtered from the precipitated sulphuret of mercury. The excess of sulphuretted hydrogen was removed by agitation with carbonate of lead, and the filtered liquid was mixed with an excess of sulphuric acid, filtered again from the sulphate of lead and distilled. The distillation was repeated and the distillate was then boiled with carbonate of silver, filtered and evaporated in vacuo. A residue was left consisting of small white crystalline grains, which repelled water just as if they contained fatty matter. When a portion of this residue was mixed with alcohol and sulphuric acid and the mixture was boiled, a smell like that of butyric ether was given off. The quantity obtained was just sufficient for one analysis, the results of which were as follows:—

0.4420 grm. gave 0.2370 grm. carbonic acid and 0.0925 water.

0.0990 grm. gave 0.0835 grm. chloride of silver.

These numbers correspond in 100 parts to

Carbon	14.62
Hydrogen	2.32
Oxygen	14.87
Oxide of Silver	68.19
	100.00

This composition approximates, as will be seen, to that of acetate of silver, which consists in 100 parts of

Carbon	14.37
Hydrogen	1.79
Oxygen	14.38
Oxide of Silver	69.46
	100.00

The excess in the amount of carbon and hydrogen and the deficiency in that of the oxide of silver show however that it must have contained a small quantity of the silver salt of another acid belonging to the same series as formic and acetic acids, a series having the general formula $C_n H_n O_4$. This acid was probably propionic acid, an acid, the formation of which must indeed be assumed, in order to explain how one of the other products of decomposition of indican takes its rise. The quantity of this acid however contained in the silver salt the analysis of which has just been given was very small, since as may be inferred from the composition of the salt, it was to that of the acetic acid in the proportion of 1 equivalent of the former to 23 equivalents of the latter.

Having described all the products to which the decomposition of indican with acids gives rise, it will now be possible to give an account of the manner in which these various products are formed and of the relation in which they stand to one another.

The decomposition of indican, after taking up several equivalents of water, into 1 equivalent of indigo-blue or indirubine and 3 equivalents of indiglucine will be evident at once from a comparison of the formulæ of these bodies.

The formation of leucine will also be easily understood when it is considered that indigo-blue and 10 equivalents of water, contain the elements of 1 equivalent of leucine, 1 equivalent of formic acid, and 2 equivalents of carbonic acid, as the following equation will show:—

$$\left.\begin{array}{ll} \text{1 eq. Indigo-blue} & C_{16}\,H_5\;\;NO_2 \\ \text{10 eqs. Water} & \;\;\;\;\;\;H_{10}\;\;O_{10} \\ \hline & C_{16}\,H_{15}\,NO_{12} \end{array}\right\} = \left\{\begin{array}{ll} C_{12}\,H_{13}\,NO_4 & \text{1 eq. Leucine.} \\ C_2\;H_2\;\;\;\;O_4 & \text{1 eq. Formic acid} \\ C_2\;\;\;\;\;\;\;\;\;\;O_4 & \text{2 eqs. Carb. acid.} \\ \hline C_{16}\,H_{15}\,NO_{12} & \end{array}\right.$$

Such a decomposition as this can of course only take place before the elements of indican have arranged themselves in such a manner as to form indigo-blue, which is a body of far too stable a nature to undergo any decomposition by the action of dilute acids.

In order to explain the formation of indifulvine it is necessary to take into consideration the simultaneous formation of formic acid. Indican may be supposed, after taking up 5 equivalents of water, to split up into 1 equivalent of *a* indifulvine, 2 equivalents of indiglucine, and 3 equivalents of formic acid, as will be seen from the following equation:—

$$\left.\begin{array}{ll} \text{1 eq. Indican} & C_{52}\,H_{31}\,NO_{34} \\ \text{5 eqs. Water} & \;\;\;\;\;\;H_5\;\;\;O_5 \\ \hline & C_{52}\,H_{36}\,NO_{39} \end{array}\right\} = \left\{\begin{array}{ll} C_{22}\,H_{10}\,NO_3 & \text{1 eq. } a \text{ Indifulvine.} \\ C_{24}\,H_{20}\;\;\;O_{24} & \text{2 eqs. Indiglucine.} \\ C_6\;H_6\;\;\;\;O_{12} & \text{3 eqs. Formic acid.} \\ \hline C_{52}\,H_{36}\,NO_{39} & \end{array}\right.$$

The following equation shows how the other modification of indifulvine may be supposed to take its rise:—

$$\left.\begin{array}{ll} \text{2 eqs. Indican} & C_{104}\,H_{62}\,N_2\,O_{68} \\ \text{7 eqs. Water} & \;\;\;\;\;\;\;H_7\;\;\;\;\;\;O_7 \\ \hline & C_{104}\,H_{69}\,N_2\,O_{75} \end{array}\right\} = \left\{\begin{array}{ll} C_{44}\,H_{19}\,N_2\,O_3 & \text{1 eq. } b \text{ Indifulvine.} \\ C_{48}\,H_{40}\;\;\;\;\;O_{48} & \text{4 eqs. Indiglucine.} \\ C_{10}\,H_{10}\;\;\;\;\;O_{20} & \text{5 eqs. Formic acid.} \\ C_2\;\;\;\;\;\;\;\;\;\;\;\;\;\;\;O_4 & \text{2 eqs. Carb. acid.} \\ \hline C_{104}\,H_{69}\,N_2\,O_{75} & \end{array}\right.$$

It may further be assumed that 1 equivalent of indican after taking up 4 equivalents of water, is decomposed into 1 equivalent of indihumine, 2 equivalents of indiglucine, 1 equivalent of propionic acid, and 2 equivalents of carbonic acid, as follows:—

$$\left.\begin{array}{ll} \text{1 eq. Indican} & C_{52}\,H_{31}\,NO_{34} \\ \text{4 eqs. Water} & \quad\;\; H_4 \quad O_4 \\ \hline & C_{52}\,H_{35}\,NO_{38} \end{array}\right\} = \left\{\begin{array}{lll} C_{20}\,H_9\,NO_6 & \text{1 eq.} & \text{Indihumine.} \\ C_{24}\,H_{20}\quad O_{24} & \text{2 eqs.} & \text{Indiglucine.} \\ C_6\,H_6\quad O_4 & \text{1 eq.} & \text{Propionic acid.} \\ C_2\qquad\; O_4 & \text{2 eqs.} & \text{Carbonic acid.} \\ \hline C_{52}\,H_{35}\,NO_{38} & & \end{array}\right.$$

The formation of indifuscone is quite analogous to that of indihumine, the propionic acid in the preceding equation being simply replaced by acetic acid, for

$$\left.\begin{array}{ll} \text{1 eq. Indican} & C_{52}\,H_{31}\,NO_{34} \\ \text{3 eqs. Water} & \quad\;\; H_3 \quad O_3 \\ \hline & C_{52}\,H_{34}\,NO_{37} \end{array}\right\} = \left\{\begin{array}{lll} C_{22}\,H_{10}\,NO_5 & \text{1 eq.} & \text{Indifuscone.} \\ C_{24}\,H_{20}\quad O_{24} & \text{2 eqs.} & \text{Indiglucine.} \\ C_4\,H_4\quad O_4 & \text{1 eq.} & \text{Acetic acid.} \\ C_2\qquad\; O_4 & \text{2 eqs.} & \text{Carbonic acid.} \\ \hline C_{52}\,H_{34}\,NO_{37} & & \end{array}\right.$$

The manner in which indifuscine takes its rise from indican needs no explanation, since a comparison of its formula $C_{24}\,H_{10}\,NO_9$ with that of indifuscone shows that its composition differs from that of the latter by containing in addition the elements of 2 equivalents of carbonic acid. In its conversion into indiretine, indican splits up into 1 equivalent of the latter body, 1 equivalent of indiglucine, and 4 equivalents of carbonic acid. Here however the anomaly presents itself of a copulated body like indican losing water instead of taking it up during its decomposition into simpler compounds, as will be seen from the following equation:—

$$\text{1 eq. Indican} \quad C_{52}\,H_{31}\,NO_{34} = \left\{\begin{array}{lll} C_{36}\,H_{17}\,NO_{10} & \text{1 eq.} & \text{Indiretine.} \\ C_{12}\,H_{10}\quad O_{12} & \text{1 eq.} & \text{Indiglucine.} \\ C_4\qquad\; O_8 & \text{4 eqs.} & \text{Carbonic acid.} \\ \quad\; H_4\quad O_4 & \text{4 eqs.} & \text{Water.} \\ \hline C_{52}\,H_{31}\,NO_{34} & & \end{array}\right.$$

It will be observed that when indican is converted into indigo-blue or indirubine it yields at the same time 3 equivalents of indiglucine, whereas the formation of the other products of decomposition is accompanied by the elimination of no more than 1 or 2 equivalents of that substance. Hence it may be inferred that the appearance of these other products is due to a part of the indiglucine undergoing a further decomposition from the action of the acid, its elements together with the residual portion of the indican affording the materials out of which the other products are formed. In fact we may easily suppose indiglucine, or perhaps more strictly speaking the group of atoms contained in indican which goes to form indiglucine, to split up into 1 equivalent of propionic acid, 1 equivalent of acetic acid, and 2 equivalents of carbonic acid, for

$$C_{12} H_{10} O_{12} = C_6 H_6 O_4 + C_4 H_4 O_4 + 2 C O_2$$

Each of these subordinate groups of atoms or any two of them may then be supposed to enter into combination with that portion of the indican which goes to form indigo-blue or indirubine and which may be called its central nucleus. When for instance indihumine is formed, 2 equivalents of indiglucine are produced from the indican, whereas the third equivalent splits up into acetic acid, propionic acid, and carbonic acid. The two latter are set at liberty and may be found among the volatile products of decomposition but the elements of the acetic acid unite with the indigo-blue group of atoms yielding by the combination indihumine. In the case of indifuscone the acetic and carbonic acids derived from the third equivalent of indiglucine are set at liberty, whereas the propionic acid combines with the indigo-blue molecule constituting indifuscone. Indifuscine again may be supposed to consist of indigo-blue, propionic acid and carbonic acid, acetic acid alone being in this case disengaged. In the process of decomposition which leads to the formation of indiretine, only 1 equivalent of indiglucine is eliminated, and 4 equivalents of

carbonic acid are disengaged, while the 2 equivalents of propionic acid as well as the 2 equivalents of acetic acid derived from the other two equivalents of indiglucine unite with the indigo-blue nucleus to produce indiretine. When *a* indifulvine is formed it must be assumed that 1 equivalent of indiglucine after taking up 2 equivalents of water, splits up into 3 equivalents of formic acid and a body represented by the formula $C_6\ H_6\ O_2$, as follows:—

$$C_{12}\ H_{10}\ O_{12} + 2\ H\ O = 3\ C_2\ H_2\ O_4 + C_6\ H_6\ O_2.$$

Now the last is the formula belonging to the aldehyde of propionic acid and by adding to this formula that of indigo-blue the sum will represent the composition of *a* indifulvine. The more complicated formula given above for *b* indifulvine, viz. $C_{44}\ H_{19}\ N_2\ O_3$ represents a compound of 2 equivalents of indigo-blue with a body whose formula is $C_{12}\ H_{12}\ O_2$ and which is therefore homologous with propylic aldehyde, its origin being due to 2 equivalents of indiglucine being decomposed in such a manner as to give rise to formic acid and carbonic acid in accordance with the following equation:—

$$2\ C_{12}\ H_{10}\ O_{12} + 2\ H\ O = 5\ C_2\ H_2\ O_4 + 2\ C\ O_2 + C_{12}\ H_{12}\ O_2.$$

It will be observed in all these cases, with one exception, viz. that of indihumine, that in the assumed combination of the elements of indigo-blue with those of these various acids &c., one or more equivalents of water must be supposed to be eliminated, as will be seen by a glance at the following equations:—

Indihumine.		Indigo-blue.		Acetic acid.
$C_{20}\ H_9\ NO_6$	$=$	$C_{16}\ H_5\ NO_2$	$+$	$C_4\ H_4\ O_4$
Indifuscone.		Indigo-blue.		Propionic acid.
$H_{22}\ H_{10}\ NO_5$	$=$	$C_{16}\ H_5\ NO_2$	$+$	$C_6\ H_6\ O_4 - H\ O$
Indifuscine.		Indigo-blue.		Propionic acid.
$C_{24}\ H_{10}\ NO_9$	$=$	$C_{16}\ H_5\ NO_2$	$+$	$C_6\ H_6\ O_4 + 2\ C\ O_2 - H\ O$
a Indifulvine.		Indigo-blue.		Propylic aldehyde.
$C_{22}\ H_{10}\ NO_3$	$=$	$C_{16}\ H_5\ NO_2$	$+$	$C_6\ H_6\ O_2 - H\ O$

b Indifulvine.		Indigo-blue.			
$C_{44}H_{19}N_2O_3$	=	$2C_{16}H_5NO_2$	+	$C_{12}H_{12}O_2 - 3HO$	

Indiretine.		Indigo-blue.		Acetic acid.		Propionic acid.
$C_{36}H_{17}NO_{10}$	=	$C_{16}H_5NO_2$	+	$2C_4H_4O_4$	+	$2C_6H_6O_4 - 8HO$

It must not for a moment be supposed that these bodies really are compounds of indigo-blue, or that the latter is in any shape contained in them or may be obtained by their decomposition. Indeed all my experiments lead to the conclusion that the elements are arranged in a manner very different from what might be inferred from the above equations. If the nitrogenous substances formed from indican together with indigo-blue were copulated bodies containing the latter, it would be possible to obtain from them either indigo-blue itself or its products of decomposition. With the small quantities of these substances which were at my disposal I was unable to make many experiments to decide this point. A tolerably large quantity of indifuscine was however subjected to the action of a strong caustic soda lye, the liquid being boiled until it left a thick mass which was heated still further, until a small portion of it no longer dissolved in water with a dark brown colour. I was unable however to discover among the products of decomposition a trace of anthranilic acid, which would probably have been present, if indifuscine contained the elements of indigo-blue.

Some advantage may nevertheless arise from looking at these compounds from the point of view just presented, as their relation to one another, to indigo-blue and to indican is thereby more vividly impressed on the memory. This method of considering them may also serve to show that these compounds are all produced at the expense of indigo-blue, that the elements contained in indican which have formed a certain portion of indifulvine, indihumine, &c., might under certain unknown circumstances have produced equivalent quantities of indigo-blue, and that the latter cannot therefore be said in any sense to pre-exist in indican.

Action of Alkalies on Indican.

In the first part of this paper I have described in general terms the effect produced on indican by alkalies. I shall now proceed to give a more detailed account of this process of decomposition and of the products, to which it gives rise.

When a watery solution of indican is mixed with caustic soda it turns of a dark yellow colour, but no further apparent change takes place. If however, after the mixture has been left to stand for several days, a portion of it be mixed with an excess of sulphuric acid and boiled, it deposits dark flocks, which after being collected on a filter and washed are found to contain no indigo-blue and to be entirely soluble in boiling alcohol. The alcoholic solution has a fine purple colour and gives only a slight precipitate with acetate of lead. Hence it follows that by the action of the alkali indican is converted into a body which by decomposition with acids yields indirubine. This body may be prepared in the following manner. A watery solution of indican having been mixed with baryta water is left to stand, until a portion of it on being boiled with an excess of muriatic acid no longer yields indigo-blue but only indirubine. The baryta is then precipitated with sulphuric acid, the excess of the latter is removed by means of carbonate of lead, the liquid is filtered and after sulphuretted hydrogen has been passed through it, it is filtered again from the precipitated sulphuret of lead and then evaporated by means of a current of air in the apparatus above described. The dark yellow syrup left after evaporation is treated with alcohol in which a great part dissolves, and the alcoholic solution is then mixed with twice its volume of ether, which causes a milkiness and produces a syrupy deposit consisting chiefly of indiglucine. The liquid after it has become clear is evaporated spontaneously when it leaves a yellow transparent glutinous residue, having a bitter taste, which cannot be distinguished in outward appearance from indican itself. This residue when dissolved in water and

treated with acid still gives indirubine, in a state of tolerable purity.

On attempting however to prepare this substance on a somewhat larger scale I found it difficult to arrest the process at this stage. As soon as the solution ceased to give indigo-blue with acids, it began to yield with acids a mixture of indirubine and indiretine, and at length it gave indiretine only, after which no further change took place. By allowing a watery solution of indican mixed with baryta water to stand until the decomposition had arrived at its last stage and then treating the solution in the way just described, a substance resembling the preceding was obtained, in the form of a brown syrup, to which I propose to give the name of

Indicanine.

This substance has the following properties. Its taste is bitter like that of indican. When heated on platinum it swells up very much and burns leaving a bulky carbonaceous residue. When heated in a tube it gives fumes, condensing to a brown liquid, which after some time becomes filled with a quantity of white crystalline needles. It is perfectly soluble in alcohol and ether. The alcoholic solution gives with an alcoholic solution of acetate of lead a bright sulphur-yellow precipitate which dissolves when more acetate of lead is added and the liquid is boiled, forming a yellow solution, in which ammonia again produces a yellow precipitate like the first. The watery solution gives only a slight precipitate with acetate of lead, but the filtered liquid yields a copious yellow precipitate on the addition of ammonia. When the watery solution is mixed with sulphuric acid and boiled it slowly deposits a quantity of brown resinous particles, which are entirely soluble in caustic soda and consist of indiretine and a little indifuscine. On adding caustic soda to a watery solution of indicanine it becomes dark yellow and on being boiled disengages ammonia,

but exhibits no further change. The analysis of the lead compound, prepared by adding acetate of lead to the alcoholic solution, filtering and washing with alcohol, yielded the following results:—

0.7840 grm. dried first in vacuo and then at 100° C. burnt with oxide of copper and chlorate of potash gave 0.6115 grm. carbonic acid and 0.1480 water.

1.0350 grm. gave 0.2225 grm. chloride of platinum and ammonium.

0.3785 grm. gave 0.3010 grm. sulphate of lead.

Hence was deduced the following composition:—

	Eqs.		Calculated.	Found.
Carbon	40	240	21.06	21.27
Hydrogen	23	23	2.01	2.09
Nitrogen.........	1	14	1.22	1.35
Oxygen	24	192	16.88	16.78
Oxide of Lead.	6	670.2	58.83	58.51
		1139.2	100.00	100.00

After deducting the oxide of lead the amount of the other constituents in 100 parts as compared with the calculated composition is as follows:—

	Eqs.		Calculated.	Found.
Carbon............	40	240	51.17	51.26
Hydrogen	23	23	4.90	5.03
Nitrogen	1	14	2.98	3.25
Oxygen.	24	192	40.95	40.46
		469	100.00	100.00

In the first part of this paper I gave an analysis of the lead compound of a substance having the formula $C_{40}\,H_{26}\,NO_{27}$ which differed therefore in composition from this merely by containing the elements of 3 equivalents more of water. As it was impossible to analyse these substances in an uncombined state, there were no means of ascertaining whether in that state they had the same composition, as was most probably the case.

Indicanine is formed from indican simply by the latter taking up water and losing 1 equivalent of indiglucine, as will be seen from the following equation:—

$$\left.\begin{array}{ll}\text{1 eq. Indican} & C_{52}\,H_{31}\,NO_{34} \\ \text{2 eqs. Water} & \underline{\phantom{C_{52}}\,H_{2}\,O_{2}} \\ & C_{52}\,H_{33}\,NO_{36}\end{array}\right\} = \left\{\begin{array}{ll}C_{40}\,H_{23}\,NO_{24} & \text{1 eq. Indicanine.} \\ \underline{C_{12}\,H_{10}\,O_{12}} & \text{1 eq. Indiglucine.} \\ C_{52}\,H_{33}\,NO_{36} & \end{array}\right.$$

The indiglucine formed in the process is contained in the brown syrupy deposit which falls on adding ether to the alcoholic solution of the indicanine. Some of this deposit, after the liquid had been poured off, was dissolved again in alcohol, the solution was mixed with an excess of alcoholic solution of acetate of lead, which produced a brown glutinous precipitate, and to the filtered liquid was added an excess of ammonia, which gave a bulky sulphur-yellow precipitate. This precipitate was collected on a filter, washed with water and decomposed with sulphuretted hydrogen, and the filtered liquid was agitated with animal charcoal until it had lost the yellowish tint which it possessed at first. The liquid having been again filtered was mixed with acetate of lead and ammonia, which produced a milk-white precipitate. This precipitate after being filtered off was redissolved in a mixture of alcohol and acetic acid, and by the addition of a small quantity of ammonia a white precipitate was again produced, which was filtered off and washed with alcohol.

1.0230 grm. of this precipitate dried in vacuo gave 0.4780 grm. carbonic acid and 0.1510 water.

0.6095 grm. gave 0.5780 grm. sulphate of lead.

In 100 parts it contained therefore

Carbon	12.74
Hydrogen	1.64
Oxygen	15.85
Oxide of Lead	69.77
	100.00

If the oxide of lead, the amount of which stands in no

simple relation to that of the other constituents, be deducted, the composition of the body combined with it will be represented by the formula $C_{12}\ H_9\ O_{11}$, which is that of anhydrous indiglucine, as will be seen from the following calculation:—

	Eqs.		Calculated.	Found.
Carbon............	12	72	42.60	42.14
Hydrogen.........	9	9	5.32	5.42
Oxygen...........	11	88	52.08	52.44
		169	100.00	100.00

The manner in which indiretine and indifuscine are formed from indicanine needs no explanation, since the composition of the latter differs from that of indican merely by the elements of 1 equivalent of indiglucine. It is however difficult to explain why indicanine by decomposition with acids should yield only these products and no indigo-blue, indirubine, or indifulvine, which might, as far as their composition is concerned, be produced at the same time, and I am quite unable to assign any cause for this phenomenon. It seems to me very probable that the indiretine and indifuscine which are formed when pure indican in large quantities is decomposed with acids owe their origin to the conversion of a portion of the indican into indicanine, before the acid has had time to effect the more complete decomposition of this portion into indigo-blue or indirubine and indiglucine.

Indican is decomposed when its watery solution is heated for a length of time, in exactly the same manner as by means of alkalies. After the solution has been heated for some time it no longer gives any indigo-blue when a portion of it is boiled with sulphuric acid. If it be now evaporated in the same apparatus as that used for evaporation of solutions of indican it leaves a brown syrup, a great part of which dissolves in alcohol. On adding ether to the alcoholic solution a syrupy deposit of indiglucine is produced followed by the separation of crystals of leucine. If the liquid be filtered and evaporated it leaves a brown glutinous residue having

the properties of indicanine. The lead compound which was obtained in the form of a sulphur-yellow precipitate by adding acetate of lead to the alcoholic solution, was after being filtered off and washed with alcohol submitted to analysis when it gave the following results:—

1.2580 grm. dried first in vacuo and then in the waterbath, gave 0.8320 grm. carbonic acid and 0.2040 water.

1.5655 grm. gave 0.2740 grm. chloride of platinum and ammonium.

0.7620 grm. gave 0.6610 grm. sulphate of lead.

These numbers correspond in 100 parts to

Carbon	18.03
Hydrogen	1.80
Nitrogen	1.09
Oxygen	15.26
Oxide of Lead	63.82
	100.00

The oxide of lead being deducted, the substance combined with it was found to have a composition agreeing with the formula $C_{40}\ H_{24}\ NO_{25}$, as will be seen by a comparison of the calculated composition with that found by experiment:—

	Eqs.		Calculated.	Found.
Carbon	40	240	50.20	49.85
Hydrogen	24	24	5.02	4.98
Nitrogen	1	14	2.92	3.01
Oxygen	25	200	41.86	42.16
		478	100.00	100.00

When a watery solution of indican or indicanine is evaporated in contact with the air, either spontaneously or with the assistance of heat a portion of it is always converted into a substance which is insoluble not only in ether but also in alcohol. That the formation of this substance is due to the action of oxygen on indicanine is proved by analysis. Its formation moreover is promoted by heating the solution of indicanine with peroxide of lead, the filtered liquid after

the dissolved lead has been removed with sulphuretted hydrogen leaving on evaporation a residue which is insoluble in alcohol. It differs however in composition according as the solution of indican has been evaporated spontaneously or with the assistance of heat. The body which is formed when a watery solution of indican is spontaneously evaporated in contact with the air, I propose to call

Oxindicanine.

So much of this body is produced during the preparation of indican, that I found it unnecessary to prepare it purposely. When the residue left after the evaporation of the watery solution of indican by means of a current of air, as described above, is treated with cold alcohol, the greatest part of the oxindicanine formed during the process remains undissolved. It may be purified simply by dissolving it in a little water and precipitating again with a large quantity of alcohol. Its appearance is that of a brown glutinous substance, which on being left to stand over sulphuric acid becomes almost dry and assumes the appearance of gum. It is insoluble in absolute alcohol and only slightly soluble in dilute alcohol. When heated on platinum it swells up very much and burns leaving a considerable carbonaceous residue. It yields when heated in a tube strong smelling fumes but only a slight trace of crystalline sublimate. Its taste is nauseous but not bitter. Its watery solution gives with acetate of lead a copious dirty yellow precipitate, and the filtered liquid gives a pale primrose-yellow precipitate on the addition of ammonia or of a large excess of alcohol. When the watery solution is mixed with sulphuric acid and boiled it slowly deposits brown flocks, which have the properties of indifuscine, while the liquid contains indiglucine. For the purpose of determining its composition I employed the lead compound, prepared by adding acetate of lead to the watery solution, filtering and washing with water.

I. 0.8515 grm. of this precipitate dried first in vacuo and then at 100° C. gave 0.7310 grm. carbonic acid and 0.1735 water.

1.1640 grm. gave 0.2575 grm. chloride of platinum and ammonium.

0.5185 grm. gave 0.3460 grm. sulphate of lead.

II. 1.2640 grm. of another preparation gave 1.0495 grm. carbonic acid and 0.2430 water.

1.5500 grm. gave 0.2505 grm. chloride of platinum and ammonium.

0.7730 grm. gave 0.5250 grm. sulphate of lead.

Hence the composition in 100 parts was as follows:—

	I.	II.
Carbon	23.41	22.64
Hydrogen	2.26	2.13
Nitrogen	1.38	1.01
Oxygen	23.85	24.25
Oxide of Lead	49.10	49.97
	100.00	100.00

After deducting the oxide of lead the first analysis gives a composition agreeing with the formula $C_{40}\ H_{22}\ NO_{31}$, whereas the second leads to the formula $C_{40}\ H_{23}\ NO_{32}$, as is shown by a comparison of the calculated numbers with those deduced from the above analyses.

	Eqs.		Calculated.	I.	Eqs.		Calculated.	II.
Carbon	40	240	45.80	45.99	40	240	45.02	45.25
Hydrogen	22	22	4.19	4.44	23	23	4.31	4.25
Nitrogen	1	14	2.67	2.71	1	14	2.62	2.01
Oxygen	31	248	47.34	46.86	32	256	48.05	48.49
		524	100.00	100.00		533	100.00	100.00

If the second formula be adopted as the correct one it follows that indicanine is simply converted into oxindicanine by taking up 8 equivalents of oxygen. The formation of indifuscine from oxindicanine takes place in consequence of the separation from the latter of 1 equivalent of indiglucine, 4

equivalents of carbonic acid, and 3 equivalents of water, in accordance with the following equation:—

$$1\text{ eq. Oxindicanine } C_{40}H_{23}NO_{32} = \left\{ \begin{array}{llll} C_{24} & H_{10} & NO_9 & \text{1 eq. Indifuscine.} \\ C_{12} & H_{10} & O_{12} & \text{1 eq. Indiglucine.} \\ C_4 & & O_8 & \text{4 eqs. Carbonic acid.} \\ & H_3 & O_3 & \text{3 eqs. Water.} \\ \hline C_{40} & H_{23} & NO_{32} & \end{array} \right.$$

The acetic acid which is produced when indifuscine is formed from indicanine does not make its appearance in this case. Indeed the 8 equivalents of oxygen which indicanine absorbs in its conversion into oxindicanine is just sufficient when added to the oxygen already contained in 1 equivalent of acetic acid to convert the carbon and hydrogen of the latter into carbonic acid and water.

When a solution of indican is evaporated in contact with the air with the assistance of heat, and the residue which remains is treated with strong alcohol there is left undissolved a brown glutinous substance, which has the properties of oxindicanine but a different composition. The lead compound of this substance was prepared by dissolving the latter in water adding acetate of lead, decomposing the precipitate with sulphuretted hydrogen, adding a little acetate of lead to the filtered liquid, filtering again and precipitating completely with sugar of lead. The precipitate which was of a dirty yellow colour was filtered off, and washed first with water and then with alcohol.

I. 1.3345 grm. dried first in vacuo and then at 100° C. gave 0.9875 grm. carbonic acid and 0.2370 water.

1.5825 grm. gave 0.4305 grm. chloride of platinum and ammonium.

0.8960 grm. gave 0.6630 grm. sulphate of lead.

II. 1.3675 grm. of another preparation gave 1.0485 grm. carbonic acid and 0.2470 water.

1.5955 grm. gave 0.4325 grm. chloride of platinum and ammonium.

0.8895 grm. gave 0.6485 grm. sulphate of lead.

These numbers lead to the following composition:—

	Eqs.		Calculated.	I.	II.
Carbon......	28	168	20.27	20.18	20.91
Hydrogen...	16	16	1.93	1.97	2.00
Nitrogen....	1	14	1.68	1.70	1.70
Oxygen.....	23	184	22.22	21.71	22.35
Oxide of Lead	4	446.8	53.90	54.44	53.04
		828.8	100.00	100.00	100.00

The following table shows the composition of the substance after deducting the oxide of lead as compared with that required by theory:—

	Eqs.		Calculated.	I.	II.
Carbon......	28	168	43.97	44.29	44.52
Hydrogen...	16	16	4.18	4.32	4.25
Nitrogen....	1	14	3.66	3.73	3.62
Oxygen.....	23	184	48.19	47.66	47.61
		382	100.00	100.00	100.00

This body may for the sake of distinction be called *Oxindicasine.* It is formed from oxindicanine by the latter taking up water and losing 1 equivalent of indiglucine, since

$$\left.\begin{array}{ll}\text{1 eq. Oxindicanine} & C_{40}H_{23}NO_{32}\\ \text{3 eqs. Water} & \underline{\phantom{C_{40}}H_{3}O_{3}}\end{array}\right\} = \left\{\begin{array}{ll}C_{28}H_{16}NO_{23} & \text{1 eq. Oxindicasine}\\ \underline{C_{12}H_{10}O_{12}} & \text{1 eq. Indiglucine.}\end{array}\right.$$

$$C_{40}H_{26}NO_{35} \qquad C_{40}H_{26}NO_{35}$$

It is possible that there may exist a body which bears to oxindicasine the same relation that indicanine does to oxindicanine. This body would be *Indicasine,* and would differ from indicanine by containing the element of 1 equivalent of indiglucine less. The following analyses of a lead compound which was obtained as a pale yellow precipitate when a large quantity of alcohol was added to the liquid filtered from the lead compound of oxindicasine, seem to countenance the idea that such a body really exists.

I. 0.9705 grm. of this precipitate, after being completely washed with alcohol and then dried, at first in vacuo and then

at 100° C. gave 0.5740 grm. carbonic acid and 0.1645 water.

1.2145 grm. gave 0.1210 grm. platinum.

0.6325 grm. gave 0.5420 grm. sulphate of lead.

II. 1.3280 grm. of another preparation gave 0.7795 grm. carbonic acid and 0.2145 water.

1.5600 grm. gave 0.1435 grm. platinum.

0.8965 grm. gave 0.7715 grm. sulphate of lead.

Hence was deduced the following composition:—

	Eqs.		Calculated.	I.	II.
Carbon......	28	168	15.90	16.13	16.00
Hydrogen...	20	20	1.89	1.88	1.79
Nitrogen....	1	14	1.32	1.41	1.30
Oxygen.....	23	184	17.44	17.53	17.59
Oxide of Lead	6	670.2	63.45	63.05	63.32
		1056.2	100.00	100.00	100.00

After deducting the oxide of lead the composition in 100 parts as compared with the theoretical composition is as follows:—

	Eqs.		Calculated.	I.	II.
Carbon......	28	168	43.52	43.65	43.62
Hydrogen....	20	20	5.18	5.08	4.88
Nitrogen....	1	14	3.62	3.81	3.54
Oxygen.....	23	184	47.68	47.46	47.96
		386	100.00	100.00	100.00

Now if the formula of this substance be doubled and the formula of oxindicasine be deducted the remainder will be the formula $C_{28}\ H_{24}\ NO_{23}$, since

$$C_{56}\ H_{40}\ N_2\ O_{46} = C_{28}\ H_{16}\ NO_{23} + C_{28}\ H_{24}\ NO_{23}.$$

The body represented by the last formula and 1 equivalent of indiglucine contain together the elements of indicanine and water, for

$$C_{28}\ H_{24}\ NO_{23} + C_{12}\ H_{10}\ O_{12} = C_{40}\ H_{23}\ NO_{24} + 11\ H\ O.$$

It has therefore the composition which theory would assign to indicasine, and the substance represented by the formula

$C_{28} H_{20} NO_{23}$ is probably a mixture in equal proportions of indicasine and oxindicasine. By decomposition of the lead compound with sulphuretted hydrogen and evaporation of the filtered liquid this substance is obtained in the form of a brown syrup, which cannot be distinguished in appearance or properties from oxindicanine or oxindicasine.

I did not enter into a more minute examination of these bodies, since their formation from indican is the only point of interest in their history.

XIII.—*On the Occurrence of Indigo-blue in Urine.*

By Edward Schunck, Ph.D., F.R.S.

[*Read April 7th,* 1857.]

The occurrence of urine exhibiting various peculiar and abnormal colours is a phenomenon which has frequently attracted the attention and excited the curiosity of pathologists. Of these variously tinted urines the most remarkable and striking are the black and the blue, but they are at the same time so rare, that it has been deemed of importance to record minutely the symptoms exhibited in each case as well as the chemical and physical properties shown by the urine itself. These urines have been observed in diseases of the most different kinds, as well as in cases in which the general health seemed not to be in the least degree affected. The pigments themselves, to which the colours are due, have not until lately been subjected to any chemical examination, and great doubts still prevail regarding their true nature. The blue pigment, to which I propose to confine myself on the present occasion, has been discovered in two states. In some cases it has been found ready formed so as to impart to the urine a blue colour, but merely in a state of suspension and therefore easily separated by simple filtration, whereas in other cases it has only made its appearance when the urine was left to stand or was subjected to the action

of various reagents. In the cases described by Janus Plancus,* Prout,† Braconnot,‡ and Simon,‖ it existed in the former state. Hassall developed the blue colour by means of putrefaction, in urines exhibiting the usual appearance, while Neubauer found the same effect to be produced by the addition of acids to the urine. As regards its chemical nature, the blue colouring matter seems to have been of three kinds, as far as can be ascertained from the descriptions given by the observers, which are not always very precise. In some cases, such as those described by Julia-Fontenelle,§ and Cantu,¶ the colour was evidently caused by Prussian blue, the iron of which appears to have been derived in one case from a quantity of ink which the person had swallowed. The second kind of colouring matter has been minutely described by Braconnot, who obtained it simply by filtering the urine from the blue deposit found suspended in it. It was a dark blue powder, insoluble in water and alkalies, only slightly soluble in alcohol and yielding no crystalline sublimate when heated. From its dissolving in acids and its being reprecipitated by alkalies and other bases, Braconnot inferred that it consisted essentially of an organic base, to which he gave the name of *cyanourine*. If the substance which he examined was pure, it seems certainly to have been of a peculiar nature. Nevertheless no one has since then observed any colouring matter which could be with certainty pronounced identical with it, though instances have been met with in which the blue colour not being caused, as it seemed, by any well-known body, has been attributed to the presence of cyanourine. In the third class of cases the blue colour was produced by a

* Commentarii Instituti Bononiensis, ad Ann. 1767.
† On Stomach and Renal Diseases. 5th ed. p. 567.
‡ Annales de Chimie et de Physique. T. XXIX. p. 252.
‖ Simon's Animal Chemistry, translated by Day. Vol. II. p. 327.
§ Archives générales de Médecine. T. II. p. 104.
¶ Journal de Chimie médicale. T. IX. p. 104.

substance, which on examination of its properties and reactions was found to be indigo-blue. Prout and Simon each mention a case in which indigo-blue was deposited from urine on standing, in the shape of a blue sediment. Neubauer* observed that the urine of a young man of 18, apparently in good health, when mixed with strong acids became first purple, then blue, and deposited a blue powder, which however he could not with positive certainty identify as indigo-blue. Hassall† was the first to point out that the occurrence of indigo-blue in urine was by no means so rare a phenomenon as had previously been supposed. The specimens of urine in which Hassall discovered it were mostly of a pale straw colour and acid. On standing they became thick and turbid and changed in colour from yellow to brown then to bluish-green, while the surface became covered with a blue scum or pellicle, which was found to consist of impure indigo-blue. Hassall considers that the exposure of the urine to the oxygen of the atmosphere is essential for the formation of the colouring matter, however I shall show that this exposure is by no means necessary. He also maintains that indigo-blue does not occur in healthy urine, that its presence is accompanied with strongly-marked symptoms of deranged health, and that its formation in urine must be regarded as a strictly pathological phenomenon, conclusions which are, as will be seen, quite at variance with the results of my experiments.

Such in a few words is the present state of our knowledge on this rather obscure subject.

In my paper "On the Formation of Indigo-blue,"‡ I have shown that the colouring matter exists in plants in a very different state to what had hitherto been supposed, that it does

* Anleitung zur Analyse des Harns. S. 19.

† Proceedings of the Royal Society, Vol. VI. p. 327; and Philosophical Transactions for 1854, p. 297.

‡ Memoirs of the Literary and Philosophical Society of Manchester, Vol. XII., p. 177; and "On the Formation of Indigo-blue," in the present Vol.

not exist in them ready formed nor as reduced indigo, and that the presence of oxygen is not essential to its formation, but that it owes its origin to the presence of a peculiar substance, soluble in water, alcohol and ether which by the action of acids is decomposed into indigo-blue, to which I have given the name of Indican, also a peculiar kind of sugar and a small quantity of other products. After having investigated the properties of this substance and its products of decomposition, I conceived it to be a matter of great interest to ascertain in what state indigo-blue exists in those urines, in which its presence is not indicated by the external appearance but is only made manifest by treatment with various reagents. That such urines should contain a body resembling indican seemed indeed exceedingly probable, since the same reagents which produce indigo-blue from indican lead in most cases to the development of the blue colour in particular kinds of urine. The extreme rarity however of these kinds of urine appeared to present an insuperable obstacle to the further investigation of the subject, and I therefore resolved to astertain whether any conclusions could be arrived at from an examination of ordinary healthy urine.

When muriatic or sulphuric acid is added to urine, the mixture on being heated becomes brown and begins to deposit dark brown flocks, which increase in quantity when the heating is continued. When these flocks are filtered off, washed and dried, they form a compact dark brown mass, from which cold alcohol extracts a resinous matter, leaving undissolved a brown powder, which dissolves however in a boiling mixture of alcohol and ammonia. This powder contains nitrogen and so much resembles indifuscine, one of the products of the decomposition of indican, as almost to lead one to suspect their identity. Its composition however, though it stands, as I have ascertained, in a certain relation to that of indigo-blue, is quite different from that of indifuscine. Now if the liquid filtered from these flocks be mixed with a salt of oxide of

copper and an excess of caustic soda, it becomes greenish, and if after being filtered it be heated for some time it gradually deposits a tolerably large quantity of suboxide of copper, which is a proof of the presence of sugar. That the latter has been formed during the process and did not pre-exist, may be ascertained by previously heating a portion of the urine with a salt of copper and caustic soda, before treating the remainder of it with acid. Samples of urine, which, when tried in this way, afforded very doubtful or no indications of their containing sugar, were found after being boiled with acid, then filtered and made alkaline, to reduce oxide of copper in a very marked manner. This reaction, which is so simple that it is only surprising it should never before have been observed, seems to me to prove that there is contained in urine some body, which by decomposition with acids yields sugar, the brown flocks precipitated at the same time being probably the substance with which the sugar was originally associated in the form of a copulated compound. From various considerations, which I need not detail, I was led to infer that this body could be no other than the very imperfectly known, so-called extractive matter of urine, and I accordingly commenced an investigation of this substance, which has led to conclusions of considerable interest. On discovering that the composition of the brown flocks formed by the action of strong acids on urine is expressed by the formula $C_{14}\ H_7\ NO_4$, which is also that of anthranilic acid, a product of the decomposition of indigo-blue, no further considerations were necessary to induce me to proceed with the investigation, notwithstanding the difficulties which I found attending it. Into the details of this investigation I shall at present only enter so far as they relate to the occurrence of indigo-blue in urine.

When acetate of lead is added to urine it produces a cream-coloured precipitate, which consists of chloride, sulphate, phosphate, and urate of lead, and contains also a little of the extractive matter of urine, which is, as it were, merely attached

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cold. After the whole of the oxide of lead has combined ne acid employed, the liquid is filtered. When there is of the indigo-producing body present the filter acquires : tinge, small particles of blue pigment are seen dotting urface of the sulphate or chloride of lead, and the surface e liquid, which is of a brownish-purple colour, in a very t time becomes covered with a thin pellicle, which is blue ransmitted and copper-coloured by reflected light, particles he same blue substance being at the same time found iched to the sides of the vessel. When there is less of the igo-producing body present, this pellicle only appears after me time, sometimes not until the next day. After twenty-ur hours however the action of the acid is always completed, that if no indigo-blue then appears or can be detected on xamination of the deposit, the total absence of the indigo-producing body may be inferred. On the succeeding day, however large the quantity of blue deposit formed may be, the liquid no longer appears purplish, but brown, and after being filtered and boiled deposits a dark brown powder, having exactly the same appearance as that produced by the action of acids on the ordinary extractive matter of urine. The matter left on the filter after being washed is treated with caustic soda, which dissolves a portion acquiring thereby a brown colour. The portion which remains undissolved after being again collected on a filter and washed, is treated with boiling alcohol. In most cases the alcohol acquires thereby a bright blue colour. When however the quantity of deposit formed is tolerably large, the boiling alcohol first dissolves another substance, which imparts to it a fine purple colour, and which I consider to be identical with indirubine.* That which the boiling alcohol leaves undissolvd is a bright blue powder having the properties of indigo-blue.

* It is very probable that Heller's urrhodine, as well as Golding Bird's purpurine are also identical with indirubine, which, as I have shown, has the same composition as indigo-blue.

It dissolves in an alkaline solution of protoxide of tin, and the solution on exposure to the air becomes covered with a blue film. It is soluble in concentrated sulphuric acid forming a blue solution, which remains blue even after dilution with water. It imparts to boiling alcohol a bright blue colour, and the solution on cooling and standing deposits blue flocks. When heated in a tube it gives a purple vapour which forms on the colder parts of the tube a blue sublimate.

Provided with this test I proceeded to examine the urine of a number of individuals, and I succeeded in obtaining indigo-blue in so great a number of instances, that I have no hesitation in saying that the indigo-producing body, if not exactly one of the normal constituents of urine, occurs more frequently than any other of the abnormal ones. The urines containing it exhibit no remarkable or peculiar appearance whatever; they are acid, clear, and of the usual colour. Its occurrence, at least if its quantity is moderate, is not to be considered as a pathological phenomenon. I can at all events state from my own experience, that its presence is not attended by any symptoms of ill health or feelings of discomfort, and that neither from the state of the health nor the appearance of the urine can any conclusions be drawn as to its presence or absence. The small number of samples of morbid urine, which I had an opportunity of examining, yielded, with one exception, no more indigo-blue than the generality of healthy urines. Nevertheless, there are no doubt diseases in which the quantity of the indigo-producing body may become so large as to constitute a truly morbid symptom, and it may therefore become a matter of importance and interest for the medical man to have a ready means of detecting it. The delicacy of the test which I have described, as well as the small quantity of the substance usually present, may be judged of from the fact that by working for several weeks on the urine of two individuals, which contained a comparatively large quantity, I obtained one grain of indigo-blue. Even

when the amount of indigo-blue formed was very small I always found that 16 fluid ounces of urine yielded an appreciable quantity of it.

The urine of forty different individuals, all of whom were apparently in a good state of health, yielded, with one exception only, more or less indigo-blue, when examined in the manner just described. These individuals belonged to both sexes, and they were of ages varying from 7 to 55. The majority were persons of the working classes. The largest quantity of indigo-blue was obtained from the urine of a man above the age of 50, a publican by trade. The urine of a young man, aged 32, a servant in my employment, yielded almost as large a quantity. Among the rest, the urine of a young man, aged 25, an engraver, that of a clerk, aged 23, and that of a girl, aged 12, who had been a cripple from infancy, were alone remarkable for the amount of indigo-blue which they yielded. In all these cases the indigo-blue was accompanied by the substance imparting to alcohol a purple colour, and which I suppose to be indirubine. The other specimens afforded much less, sometimes mere traces. In all cases however in which the urine of the same individual was examined at different times, the amount of indigo-blue obtained from it was found to vary exceedingly, it being sometimes considerable and occasionally dwindling down to a mere trace. It was only very rarely however that none was found. In the case of the individual first referred to, the urine gave on one occasion not a trace, and this took place when he was engaged in performing labour, unusual for him both in its nature and amount. In my own case, as well as that of my assistant, the amount varied most capriciously from a tolerable quantity to a mere trace, occasionally even none at all being obtained.

I performed several experiments with different kinds of diet in order to ascertain the effect on the amount of indigo-blue yielded by the urine. Only one experiment, however, led to any decisive result. Having selected an occasion, when the

night urine gave no indigo-blue, I took on the next night, before going to bed, a mixture of treacle and arrowroot boiled with water in as large a quantity as the stomach could bear, and the effect was that the urine of the following night gave a large quantity of indigo-blue. As, however, the same phenomenon was repeated for several succeeding nights without any additional quantity of food having been taken, it remained uncertain to what cause it was to be attributed, though a repetition of the experiment on a second occasion gave the same result.

I have hitherto not had an opportunity of examining many specimens of urine in disease. Of two samples of urine from patients with albuminaria one gave a small quantity of indigo-blue, the other not a trace. Several specimens of diabetic urine yielded it. One of these, which I owed to the kindness of Dr. Browne of Manchester, gave a much larger quantity than I obtained from any other specimen of human urine.

The urine of the horse and the cow when tried in the same way as human urine gave comparatively very large quantities of indigo-blue, especially that of the horse.

I think it is highly improbable that the indigo-blue obtained in Hassall's experiments was produced, as he supposes, by the action of oxygen on the urine. Its formation was without doubt due to the decomposition of the indigo-producing body induced by the fermentation of the urine, the indigo-blue at the moment of its formation dissolving in the fermenting alkaline liquid and producing a true indigo vat, from which it was gradually deposited by the action of the atmospheric oxygen. When small quantities of indigo-blue only are formed in any specimen of urine, fermentation is not in my opinion to be recommended as a means of detecting it.

The occurrence of the indigo-producing body as an excretion seems to me to be due to a disproportion between the oxygen absorbed by the system and the matter to be acted on by it, which again may be caused either by an excessive

waste of the tissues or by an obstruction of the organs conveying oxygen, as the lungs and skin, or, as is probably the case in the majority of instances, by an excess of food being taken over and above the requirements of the system. As regards the constitution of this body, I think there can be no doubt that it contains the elements of indigo-blue and sugar, and that by oxidation within the system it is converted into the ordinary extractive matter of urine, which contains, as I have ascertained, the elements of sugar and of the black substance which is formed by the action of strong acids on urine, and which may be considered as a product of the oxidation of indigo-blue. Having prepared the extractive matter of urine in a state of purity, ascertained its composition, and examined its products of decomposition, I think it is probable that the indigo-producing body will be found, as regards its formation and composition, to occupy a place between the substance of the tissues and the ordinary extractive matter of urine. The very minute quantities of it ordinarily occurring in urine, and the difficulty of separating it from the extractive matter, make it, however, impossible to ascertain whether this is the case or not. My object in making known this portion of the investigation in its present fragmentary state, is to induce medical men, who have an opportunity of examining many varieties of urine, to endeavour to discover among these varieties some containing a sufficiently large quantity of this body to enable the chemist to ascertain its properties and composition.

The formation of a substance containing the elements of indigo-blue in the animal system is a fact which may lead to important conclusions regarding the chemical composition of the complex bodies of which the blood and tissues consist.

SESSION 1856—1857.

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